The Dissertation committee for Kori James Stroub certifies that this is the approved version of the following dissertation:

The Effect of School Closures on Student Achievement: Evidence from Houston

Committee:

____

Pedro Reyes, Supervisor

____

Richard Reddick

____

Susan Beretvas

____

Norma Cantu

____

Robert Crosnoe

____

Meredith Richards

The Effect of School Closures on Student Achievement:

Evidence from Houston

by

Kori James Stroub

Dissertation

Presented to the Faculty of the Graduate School

of the University of Texas at Austin

in Partial Fulfillment

of the Requirements

for the Degree of

Doctor of Philosophy

The University of Texas at Austin

December 2017

The Effect of School Closures on Student Achievement:

Evidence from Houston

by

Kori James Stroub, PhD

The University of Texas at Austin, 2017

SUPERVISOR: Pedro Reyes

School closures are an increasingly common reform strategy for districts facing declining enrollments and low academic performance. In this study, I examine the impact of 46 closures on the achievement of 6,826 displaced students in Houston between 2003 and 2010, comparing their achievement trajectories to those of a matched sample of non-displaced students. I find that closures are associated with a short-term increase in math achievement; however, displaced students have flatter math achievement slopes than their non-displaced peers. Cumulatively, closures have a relatively small effect on reading achievement. Finally, while closures can benefit students that transfer to high-performing campuses, few students – particularly low-achieving and non-white students – transfer to campuses of sufficiently high academic quality to produce achievement gains.

Table of Contents

List of Figures

List of Maps

List of Tables

CHAPTER 1: INTRODUCTION

Overview

Over the past decade, the closure of urban public schools has disrupted the educational experiences of hundreds of thousands of public schoolchildren. Once primarily associated with population declines in rural areas, school closures are now a key strategy for urban school reform. Indeed, school districts in cities such as Chicago, Cincinnati, Detroit, Indianapolis, Kansas City, Minneapolis, New York, Philadelphia, Pittsburgh, St. Louis, and Washington, D.C. have shuttered large numbers of schools over the past two decades (Dowdall, 2011). In the 2014-15 school year alone, urban school closures displaced over 100,000 students (Author calculations, NCES CCD).

Not surprisingly, closing public schools is a highly controversial policy option. Proponents of school closures argue that they are necessary measures to combat declining enrollments, budgetary shortfalls, and high academic failure rates (Sunderman & Payne, 2011). Indeed, enrollment in many the country's largest urban districts has declined as suburban and exurban migration and urban disinvestment have contributed to steady decreases in the number of school-aged children (Burdick-Will, Keels, & Schuble, 2013; de la Torre & Gwynne, 2009; Engberg, Gill, Zamarro, & Zimmer, 2012). In addition, the rapid proliferation of charter schools has resulted in increased competition for students, often siphoning enrollment from traditional public schools (Dowdall, 2011; Winters, 2010).

As a result of these trends, many public schools are operating below capacity. Arguing that such schools are inefficient to operate, districts contend that closure of underutilized campuses will liberate resources for other educational investments (Sunderman & Payne, 2009).

In addition to combating such economic exigencies, closures have also increasingly emerged as a reform strategy to address chronic low academic performance. Indeed, performance-based closures have been incentivized by the federal government through reauthorization of the Elementary and Secondary Education Act (ESEA) legislation and such initiatives as Race to the Top and School Improvement Grants. These initiatives explicitly incentivize closure as a means of "turning around" campuses with chronically-low test scores (Jack & Sludden, 2013). As a result, in nearly all the recent high-profile closure cases, proponents have explicitly argued that closures will benefit displaced students by rescuing them from chronically under-performing schools (Brown, 2015; Carr, 2013; Corley, 2013; Fleisher, 2013).

Critics have highlighted the significant challenges of closing schools in practice and the consequences of these closures for students, families, and communities. In particular, critics have drawn attention to the disproportionate impact of closure decisions on historically under-served student populations, particularly black and economically disadvantaged students (Corley, 2013; Duffin, 2009; Fleisher, 2013; Hurdle, 20013). Moreover, the impact of closures on disadvantaged communities may extend beyond students, as school closures may result in the laying off or transfer of teachers and other school employees and the erosion of an important social and historic institution in the neighborhood (de la Torre, 2009; Hurdle, 2013; Steiner, 2009; Sunderman, 2009). Echoing these concerns, the United States Office for Civil Rights has investigated the school closure policies of Detroit, Houston, New York, and Philadelphia (Fleisher, 2013; Hurdle, 2013).

Moreover, critics have problematized the assumption that closures will improve student achievement. By disrupting students' educational environments and peer group networks, critics argue that closures may adversely affect students' academic achievement and attainment. Contrary to the theory of action offered by policymakers, critics have further argued that closures do not necessarily result in students attending more advantaged academic contexts, as displaced students often transfer to schools that are little to no better in terms of academic performance than those they left (de la Torre & Gwynne, 2009).

The increasing frequency of closures as an urban reform strategy and the stakes of such closures for students and communities underscores the importance of research exploring the impact of closure policies. However, few empirical studies, particularly peer-refereed studies, have directly investigated the impact of closures. In this study, I evaluate the competing claims regarding the impact of school closures on student achievement using data from the Houston Independent School District (HISD) in Texas.

My findings highlight the long-term negative impact of closures on student achievement, particularly math achievement. I find that the negative impact of closures may be mitigated by transferring to a high-quality receiving school. However, a relatively small proportion of displaced students transfer to schools of sufficiently high-quality to result in higher levels of long-term achievement than their non-displaced peers. Troublingly, while Asian, White, and high-achieving students were particularly likely to transfer to high-quality schools, Black, Hispanic, and low-achieving students were much more likely to transfer to schools of low or average quality.

Study Purpose

In this study, I contribute to the emerging literature on school closures by examining the impact of closures on student achievement in HISD. Houston presents a unique context for studying closures. While the bulk of the attention to closures has centered on schools in the declining urban cores of the Northeast and Midwest, HISD relatively quietly shuttered 72 of its 374 schools between 2000 and 2014, despite rapid growth in its greater metropolitan area over the same period (U.S. Census Bureau, 2016). Moreover, rather than being triggered by chronically poor academic performance, most of the schools closed by HISD were rated “academically acceptable” under the state accountability system at the time of closure, although closed schools underperformed vis-à-vis schools that remained open.

Prior research has consistently highlighted the importance of school quality as a moderator of the effect of closures on achievement. However, extant scholarship has provided little detail on the nature of this relationship, and has produced little policy-relevant information on how many and what types of students transfer to high-quality schools or the level of school quality necessary to result in meaningful increases in achievement. Towards that end, in this study, I attend to how student achievement after closure is moderated by the quality of receiving schools to which students transfer, seeking to extend prior work to provide policy-relevant information regarding the quality of schools necessary to produce gains in achievement.

Finally, issues of race and class have been central to the public debate over closures. Prior scholarly research, however, has provided little insight into the racial dynamics of the impact of closures. Towards that end, I seek to explore racial/ethnic and other demographic differences in student transfer patterns and their differential impact on student achievement.

Research Questions

Towards addressing the primary research objectives of this study, I will explore the following four research questions:

1. *How do closures affect the short- and longer-term achievement of displaced students in HISD?*
2. *How do the effects of closures vary by the race/ethnicity and socioeconomic status of displaced students?*
3. *How is the effect of closures on achievement related to the academic performance of the receiving schools to which displaced students transfer?*
4. *How does the academic performance of receiving schools to which displaced students transfer vary by student characteristics?*

Overview of Methodology

To address the research questions outlined above, this study draws on rich student-level, administrative data from the state of Texas to construct a longitudinal dataset that links each public school student in HISD to his/her demographic characteristics, annual achievement scores, and annual schools of attendance. Moreover, by identifying elementary school closures, along with the specific years in which they occur, it is possible to track the academic progress of students as they experience closures and are subsequently reassigned to new schools. This study tracks the educational progress of all HISD students during the years that the TAKS was administered, 2002 to 2010.

This study employs a three-phase analytic strategy to assess the casual effects of school closures on the academic trajectories of displaced students. Because school closures are not assigned at random and the characteristics of students that experience closures differ systematically from students that do not experience closures, the first phase of the analysis employs propensity score matching techniques to minimize the effects of selection bias when estimating the effects of closures on student achievement. First, I estimate a series of propensity models, predicting a student’s conditional probability of experiencing a closure during the study period as a function of student and school characteristics. These conditional probabilities, or propensity scores, were then used to match each displaced student in the sample to a comparable non-displaced student. The final analytic sample consists of 6,826 students displaced by a closure and 6,826 students never displaced by a closure (N = 13,652).

Once the matched sample was constructed, in Phase II of the analysis I estimate a series of multi-level longitudinal discontinuous change models predicting the impact of school closure on the short- and longer-term academic trajectories of displaced students. The primary outcomes of interest are student's annual raw scores on the math and reading TAKS. Discontinuous change models are specifically designed to estimate the effects of a discrete event (e.g., a school closure) on individual growth trajectories (e.g., student achievement trajectories). In addition to estimating the immediate and longer-term effect of closures on academic achievement overall, I also estimate a series of interactions, testing the extent to which the effect of closures varies by the demographic characteristics of displaced students and the quality of the schools to which displaced students transfer.

Finally, in Phase III of the analysis, I compute a series of robustness indices estimating how sensitive my analyses are to omitted variable bias. Although I use propensity matching techniques to account for the systematic differences between students that experience closures and students that do not experience closures, there is no way to determine with certainty that the effects of non-random treatment assignment have been eliminated completely. Indeed, one limitation of propensity score techniques is that they can only account for measured sources of treatment bias. Robustness indices address this limitation of propensity techniques by quantifying the amount of bias, stemming from unobserved variables, that would need to be present to invalidate a causal inference.

CHAPTER 2: LITERATURE REVIEW

Overview

Previous researchers have noted that the impact of closures on student achievement is theoretically ambiguous (Brummet, 2014; Carlson & Lavertu, 2015). On one hand, closures may negatively impact the academic achievement of displaced students by disrupting their educational environments and peer group networks. Alternately, reflecting the hopes of many policymakers, closures may benefit students by liberating them from failing schools and placing them in contexts more conducive to academic success. Implicit in the debate, therefore, is the issue of policy implementation. The extent to which closure policies may benefit or harm students is, in part, a function of the quality of schools that students attend and whether the quality of their new schools are sufficient to outweigh any negative effects of mobility itself. As a result, research on school closures and student mobility has consistently focused on the moderating influence of school quality; however, as I discuss below, there has been little attention to policy-relevant issues such as the quality of schools necessary to produce higher levels of achievement or the types of students that actually access high-quality schools in practice.

Moreover, issues of race and class are nearly always at the core of ongoing debates over the relative merits and faults of closure policy. Given that the primary determinants of whether a school is at risk for closure (chronic under-enrollment and low-achievement) tend to be highly correlated with the racial/ethnic and socioeconomic composition of campuses, it is not surprising that research has consistently demonstrated that closures disproportionately impact economically disadvantaged students and students of color (Brummet, 2010; de la Torre & Gwynne, 2009; Engberg, et al., 2012). If closures have a positive impact on student achievement, then district may be able to leverage the disproportionate impact of closures to narrow long-standing racial/ethnic and socioeconomic achievement gaps. On the contrary, if closures have a negative impact on student achievement, then district utilizing closure policy will run the risk of exacerbating the very gaps they often seek to eradicate.

In the following sections, I discuss two areas of research that provide important insights into the potential effects of closures. First, I review the research on school mobility. While school closures are conceptually distant from school mobility in important ways—closures are forced upon the student, whereas mobility is typically voluntary—there are commonalities between the two phenomena that render an examination of the mobility literature worthwhile—notably, student transfer from one school to another. Second, I will review the scant on school closures. Across both literatures, I focus primarily on the disparate impact the phenomena have on students from different racial/ethnic and socioeconomic backgrounds, as well as the moderating effects of the new schools to which students are transferring on achievement outcomes.

Evidence on the Effects of Student Mobility

Although they do not focus on school closures specifically, several studies of student mobility provide insight into the potential effects of closures. A relatively large body of evidence suggests that mobility can have small to modest effects on student achievement, depending on why a move occurred, the timing of the move, and the quality of the school to which they transfer (Alexander, Entwisle, & Dauber, 1996; Hanushek, Kain, & Rivkin, 2004; Pribesh & Downey, 1999; Rumberger & Larson, 1998; Schwartz & Stiefel, 2016; Xu, Hannaway, & D'Souza, 2009).

Findings from the early literature on school mobility consistently demonstrate that school moves are associated with declines in the academic achievement of movers. Moreover, as has been suggested previously, this early body of work largely forms the basis of the conventional understanding regarding the impact that mobility has on student achievement (Hanushek, Kain, & Rivkin, 2004; Schwartz & Stiefel, 2012). However, as Mehana and Reynolds (2004) demonstrate in a recent meta-analysis of the quantitate mobility studies conducted between 1975 and 1994, this early research tends to suffer from the most severe methodological limitations. In particular, the findings from this body of work are based primarily on cross-sectional data and rely on simple comparisons of the average achievement of movers and non-movers to make inferences about the effects of mobility. Much of the early work also fails to account for heterogeneity in the types of moves students make and do not adequately control for the unobserved student and family characteristics that often precipitate a move.

In a subsequent generation of studies that employ more sophisticated longitudinal techniques and control for a wider range of student and family characteristics, the findings are much more equivocal. For instance, Alexander, Entwisle and Dauber (1996) track the educational trajectories of a sample of elementary school students in Baltimore from 1st to 5th grade. They find that after controlling for the demographic and academic backgrounds of students (including 1st grade test scores), the effects of the number of school moves on 5th grade achievement fail to reach statistical significance for math but remain statistically significant for reading. In a similar study, Temple and Reynolds (2000) follow a sample of low income black students in Chicago from kindergarten to 7th grade. In slight contrast to the Baltimore study, Temple and Reynolds find that after controlling for the background characteristics of students and academic achievement in kindergarten, the number of school moves had a significant negative effect on both 7th grade mathematics and reading achievement.

A second set of longitudinal studies using nationally representative data collected by the National Center for Education Statistics also produce mixed results. These studies all used the National Educational Longitudinal Study of 1988 (NELS88), which tracks the educational progress of students between 8th and 12th grade. As such, this set of studies is focused primarily on the effects of mobility on high school achievement. Compared to the data used in the studies discussed above, the NELS88 contains a much richer set of student and family characteristics. As such, these studies were able to control for a much wider array of background characteristics. For instance, Rumberger and Larson (1998) were able to estimate the effects of changing schools once or twice on high school graduation. This study all estimated the effect of residential moves on high school graduation. They find that after controlling for a large number of student, family, and middle and high school characteristics, all three types of moves were associated with significantly lower high school graduation rates. Similarly, Pribesh and Downey (1999) differentiated between moves that involved changing only high schools, moves that involved changing only residences, and moves that involved changing schools and residences. They find that moves involving a school and residential change are associated with the largest decline in 12th grade test scores. However, when controls were added to the models, the results remained statistically significant for mathematics but not reading. Finally, Swanson and Schneider (1999) examine the effects that the timing of a move has on mathematics achievement. They categorize moves as early, occurring before 8th grade and between 9th and 10th grades, or late, occurring between 10th and 12th grades. As with the other studies using the NELS88 data, they control for a wide range of student, family, and school characteristics. They find that early moves have a positive effect on mathematics achievement, while late moves have a negative effect on mathematics achievement.

One of the more recent and methodologically sophisticated mobility studies was conducted by Hanushek, Kain, and Rivkin (2004). This study estimated the annual gains in mathematics scores for three cohorts of Texas elementary school students between 4th and 7th grades. Importantly, this study is the only mobility study to date that employs student fixed effects to control for any unobserved time-invariant differences between mobile and non-mobile students. Findings from this study suggest that within-year moves are associated with declines in mathematics achievement while between-year moves have no statistically significant effect on gains in achievement.

Troublingly, and perhaps most relevant to the school closure context, Hanushek, Kain, and Rivkin as find that economically disadvantaged students and students of color are less likely to transfer to higher performing schools than their affluent, and white peers. Moreover, economically disadvantaged students and students of color also appear to be more sensitive to the disruptive effects of closure. That is, net of changes in school quality, the negative effect of mobility is larger for economically disadvantaged students and students of color than it is for their relatively advantaged peers.

Implications for Closures. Taken together, these findings have potentially important implications for the effect of closures on student achievement. First, that the effect of mobility is moderated by the quality of the schools to which students transfer, suggests that the impact of school closures will also depend on the quality of the receiving schools to which displaced students transfer. This insight highlights an interesting difference between student mobility and school closures. Whereas mobility is the results of a more-or-less voluntary decision on the part of parents and students, closures are essentially forced upon families by district policymakers. Indeed, districts effectively select the schools to which displaced students transfer. From a policy perspective, this represents an important opportunity for districts to minimize the negative effects of mobility, in this case precipitated by a school closure, by ensuring that displaced students are reassigned to the highest-quality school possible.

Second, the finding that economically disadvantaged students and students of color are both less likely to transfer to higher quality schools, and more vulnerable to the negative effects of mobility suggests that closures may have a particularly pernicious effect on these historically under-served populations. While districts have a certain degree of control over which schools they select as receiving schools for displaced students, the fact that the highest quality schools within a district tend to be clustered within affluent and white neighborhoods (de Souza Briggs, 2007), suggests that displaced poor and non-white students may be filtered into nearby schools that are similar to, or worse than, the school from which they were displaced.

Evidence on the Effects of School Closure

A smaller body of evidence has directly examined the effects of school closures. A handful of qualitative studies have documented the negative social consequences of closures on students, particularly related to loss of friends and peer networks (e.g., Kirshner, Gaertner & Pozzoboni, 2010; Lipman & Person, 2007; Steiner, 2009). While such factors are certainly antecedents of academic success, such qualitative work does not provide direct evidence of the impact of closures on achievement. In a unique study, Sacerdote (2012) examines the impact of forced student mobility caused by the closure of schools after Hurricanes Katrina and Rita on the subsequent achievement of students. Sacerdote finds that displaced students had short-term declines in achievement, but these effects "fade out" over time, as displaced students catch up to their non-displaced peers. However, given that the closures prompted by Katrina and Rita were also accompanied by homelessness and other forms of social displacement for students, it is difficult to disentangle the effects of closure from the broader effects of the hurricanes.

Five quantitative studies have directly examined closure policies on student achievement. In three of the five studies of closures on student achievement, scholars document short-term declines in achievement that “fade out” over time as displaced students “catch up” to their peers. In the earliest study of closures, de la Torre & Gwynne (2009) examine the impact of the closing of 38 schools in Chicago. Using a school-level propensity score matching technique to compare students in closed schools to students in comparable schools that did not close. De la Torre and Gwynne find that the closing of chronically low performing schools was associated with a short-term drop in achievement. However, they find no persistent negative effects of closures on displaced students.

In a study of 246 school closings in Michigan, Brummet (2014) finds similar results to de la Torre and Gwynne. Using a difference-in-differences (DiD) approach, Brummet finds that student math achievement declines in the year prior to closure and remains low in the year following closure. By one year after experiencing a closure, however, the math achievement of displaced students begins to improve. Brummet finds a similar pattern of results for reading achievement, although the results are not consistently statistically significant.

Likewise, in a study of 22 school closures which displaced roughly a quarter of the students in an anonymous mid-sized urban district, Engberg, Gill, Zamarro & Zimmer (2012) examine the effects of a single wave of closures using an instrumental variable technique. Like de la Torre and Gwynne (2009) and Brummet (2014), Engberg et al. find that closures negatively impacted student academic achievement in math and reading. In contrast to the foregoing studies, however, Engberg et al. find that the negative effects of closures persist for up to three years after being displaced, which was the maximum amount of time that the authors tracked students post-closure. The authors also found that the persistent negative effect of closure could be eliminated if displaced students transferred to receiving schools of sufficiently high quality. Finally, it is important to note that the closure reforms undertaken by the anonymous district studied by Engberg et al. was specifically designed to close the districts lowest-performing campuses to relocate the displaced students to higher-performing schools.

The exceptions to the trends documented above are two recent studies by Carlson and Lavertu (2015, 2016). Carlson and Lavertu (2015) examine the effects of 198 closures of traditional and charter schools in the state of Ohio. Using a DiD similar to that of Brummet (2014), they find that closures have consistently positive short- and longer-term effects on student achievement in math and reading. They also find that the quality of the receiving school is positively related to the effect of closures, such that students transferring to relatively high-performing receiving schools, exhibited particularly large treatment effects. Similarly, in a study focusing exclusively on the effects of Ohio's mandatory closure policy for failing charter schools, Carlson and Lavertu (2016) find that charter closures yielded eventual achievement gains via a regression discontinuity technique.

Since the findings of Carlson and Lavertu are somewhat anomalous when compared against the earlier work on school closures, I think it important to examine their work in slightly greater detail. As mentioned above, Carlson and Lavertu employ DiD to estimate the effect of closures on student achievement in Ohio. Unlike Brummet, who also employed a DiD, Carlson and Lavertu data violate DiD's fundamental assumption of parallel paths in the pre-treatment period (Murnane & Willett, 2011). That is, the average achievement trajectory of displaced students, prior to experiencing a closure, does not have the same slope as the average achievement trajectory of non-displaced students in their sample. While the actual difference between the two trends is relatively small, it is statistically significant. Consequently, there estimates of the treatment effect are biased by an unknown amount.

Additionally, as Kirshner and Gaertner (2015) note, when examining the moderating effects of school quality on the relationship between closures and student achievement, they limit their sample to only those student that were displaced to higher-performing schools. This selection criteria cut they sample of displaced students by 40%. Consequently, with regard to the moderating effect of school quality, Carlson and Lavertu were only able to estimate the effect of closures when students transfer to higher quality schools. Omitting students that transferred to similarly achieving and lower achieving campuses likely biases readers' interpretation of the authors findings.

Summary

While scholars have come to somewhat different conclusions regarding the effect of closures on net academic achievement in different contexts and using different methodologies, researchers have consistently linked the impact of closures on students to the quality of the schools that closed and to which displaced students transferred. Engberg et al. (2012) find that transferring to significantly higher-performing schools mitigates declines in performance, but does not result in long-term gains in achievement. Focusing on the quality of schools that closed, Brummet finds that students displaced from low-achieving schools have sharp increases in achievement after closure, while students displaced from high-achieving schools have persistently lower achievement after closure. Carlson and Lavertu (2015) find that closures had even larger positive effects on students that transferred to higher-quality schools than those they left. Although de la Torre and Gwynne (2009) do not explicitly test the impact of school quality, they conclude the lack of positive effects may be attributable to the fact that only 6% of their sample of displaced students transferred to schools in the top quartile of district achievement.

Scholars also consistently find that closures disproportionately displace economically disadvantaged students and students of color. Despite the regularity of these findings, no closure research has focused on the differential impact that closures might have on these vulnerable groups of students. This gap in the literature is particularly troubling given findings form the mobility literature suggesting that poor and non-white students are more sensitive to the negative effects of mobility on student achievement.

CHAPTER 3: METHODOLOGY

Overview

To investigate the effects of HISD's school closures on the academic achievement trajectories of displaced students, I pair administrative records from HISD regarding the timing of public school closures with longitudinal data from the Texas Education Research Center (ERC), Texas' state education data warehouse. I use the constructed longitudinal data set to track the academic progress of students as they experience closures and are displaced to their new schools. All HISD students that experienced a closure between 2003 and 2010 are included in this study.

Estimating the impact of closures on the achievement of displaced students proceeds in three phases. Because closures are not assigned to schools at random and the characteristics of students that experience closures differ systematically from students that do not experience closures, I first use propensity score matching techniques to pair displaced students with an appropriate control group of non-displaced students that are similar on an array of observed student and school characteristics. After matching, my final analytic sample includes 6,855 displaced students and 6,855 non-displaced students (N = 13,710).

In Phase II of the analysis, I estimate the impact of closures on the academic trajectories of displaced students via a series of multi-level, discontinuous change models. Discontinuous change models are specifically designed to estimate the effects of a discrete event (e.g., a school closure) on individual growth trajectories (e.g., student achievement trajectories). In addition to estimating the immediate and longer-term effect of closures on academic achievement, I also estimate a series of cross-level interactions to determine how the effect of closures is moderated by the quality of the receiving schools to which displaced students transfer.

Finally, in Phase III of the analysis, I compute a series of robustness indices for the casual effects estimated in Phase II. Although this study employs propensity score matching to control for systematic differences between students that experience closures and students that do not experience closures, propensity score techniques can only account for differences stemming from observed, or measured variables. Robustness indices address this limitation of propensity techniques by quantifying the amount of bias, stemming from unobserved variables, that would need to be present to invalidate my causal inferences. While these robustness checks cannot determine with certainty that no crucial variables have been omitted from my analyses, they do provide additional support for my causal inferences regarding the effect of closures on student achievement.

Data

To assess the effects of school closures on the academic trajectories of displaced students, I leverage the rich longitudinal data housed at the Texas Education Research Center (ERC). Created by legislative mandate in 2006, the ERC serves as the state's longitudinal education data warehouse and maintains a broad range of student-, school-, and district-level data collected from the Texas Education Agency (TEA), the Texas

Higher Education Coordinating Board (THECB), and the Texas Workforce Commission (TWC). The ERC database maintains annual records dating back to the early 1990s.

A strength of the ERC data is that it is compiled from annual administrative data for all students enrolled in public schools across Texas. As such, unlike smaller databases (e.g., data from a single district), ERC data allows me to track students if they leave HISD and transfers to a different public school district within Texas. Moreover, since the ERC links student- and organizational-level (e.g., schools and districts) data across time via unique identifiers, it is possible to track the educational trajectories of students as they progress through Texas's public school system. It is important to note, however, that unique student identifiers are only assigned to students attending public school in Texas. As such, it is not possible to track students that move out of state or that transfer to private schools.

Although the ERC houses a broad range of educational and work-force related data, this study will leverage information collected by the TEA on student, school, and district demographic and academic characteristics. This study leverages the following three TEA databases: 1) the Public Education Information Management System (PEIMS), 2) the Texas Assessment of Knowledge and Skills (TAKS) database, and 3) the Academic Excellence Indicator System (AEIS).

The PEIMS database encompasses all data requested and received by the TEA about public education in Texas. It includes data on student demographics and academic performance, personnel, finance, and the organizational characteristics of schools and districts. The PEIMS will be used to obtain all student-level demographic information required for this study. Information on student race/ethnicity, economic status, and special program status (e.g., special education and LEP) will be obtained from the PEIMS.

The TAKS database, which is derived from PEIMS data, contains the entire universe of student scores on the TAKS test. The TAKS was first implemented in the 2002-03 school year and was administered to students in grades 3 through 11 through the 2009-10 school year. The test assessed students' mastery of grade-specific subject matter in the follow five areas: reading, writing, mathematics, science, and social studies. Although students were tested in five different subjects, only mathematics and reading assessments were administered in all tested grades (i.e., grades 3 through 11). As such, to include the widest range of grade levels, and consequently the maximum number of students in my analyses, I have focused on only math and reading outcomes. Student's math and reading scores are linked to the student demographic information acquired from the PIEMS and were used to track the achievement of students as they progress through the Texas public school system.

Finally, the AEIS database, which is also partly derived from PEIMS data, contains organizational-level demographic, achievement, and accountability data. Of interest are information on school and district racial/ethnic and economic composition, aggregate TAKS achievement, and accountability status, such as if a school or district has been labeled academically unacceptable for chronically low student achievement. By merging AEIS data with PIEMS and TAKS data, it is possible to construct a longitudinal data set that links each public school student in Texas to his/her demographic characteristics, annual achievement scores, and annual school and district of attendance.

The Houston Context

As discussed previously, Houston presents a unique context for studying closures for several reasons. While most of the existing work on closures has focused on the declining urban cores of the Northeast and Midwest, such as New York, Washington, D.C., Philadelphia, Chicago, and Detroit. In contrast, Houston is one of the nations fastest growing metropolitan areas. Over the last decade, the population of Houston has increased by about 5% annually (Author Calculations, U.S. Census). Moreover, Houston is has also benefited from rapidly expanding energy and shipping industries (Sixel, 2015).

Despite rapid population growth in the metropolitan area, HISD has suffered from declining enrollments for the past several years. Indeed, over the study period, between 2002 and 2010, total enrollment in HISD declined by over 5%. Despite the decline in enrollment, HISD remains the 5th largest district in the country. Table 1 presents basic descriptive statistics for HISD.

In partial response to declining enrollment, over the past two decades, HISD has quietly shuttered over 70 schools. Moreover, unlike previously studies contexts, rather than being triggered by chronically poor academic performance, most of the schools closed by HISD were rated “academically acceptable” under the state accountability system at the time of closure, although closed schools underperformed vis-à-vis schools that remained open.

Study Variables

Table 2 lists each of the dependent and independent variables used in this study. Each of these variables is discussed in detail below, addressing the computation method, level of analysis, and interpretation of each.

Phase I—Dependent Variable

The propensity models estimate the conditional probability of experiencing a closure for each student in the sample. The dependent variable in these models is a dichotomous variable indicating if a student experienced a closure during the study period. Students experiencing a closure during the TAKS era are assigned a value of “1”, while students never experiencing a closure are assigned a value of “0”. To have experienced a closure, a student must have been enrolled in a school in the final 6-week reporting period of the school year, prior to the school closing. For instance, consider a campus that closed at the end of the 2008-09 school year. Any student enrolled in this campus during the last six weeks of the school year would be coded as experiencing a closure.

Phase I—Independent Variables

Student-level covariates. To account for any systematic differences in the types of students that experience school closures, a variety of student-level covariates were included in the propensity models.

Achievement. Students' annual, raw TAKS scores, in both reading and mathematics. The raw scores indicate the number of test questions students answered correctly. The maximum possible raw score is generally 60, however the exact value varies slightly across subjects and years. This variable comes from the TAKS database.

Age. The age, in years, of students. This variable comes the PEIMS data.

At-Risk. This variable indicates if a student is at-risk of dropping out of school. To be considered at-risk for dropping out, a student must satisfy at least one of the following state-defined criteria: 1) be in grades pre-kindergarten through 3 and perform unsatisfactory on a school readiness assessment, 2) be in grades 7 through 12 and fail to maintain an average of 70 in two or more subjects, 3) be held back a grade, 4) fail to meet the satisfactory level on a state assessment (e.g., the TAKS), 5) be pregnant or a parent, 6) be placed in an alternative education program, 7) be expelled from school, 8) be on parole, probation, or deferred prosecution, 9) be limited English proficient, 10) be in the custody of the Department of Protective and Regulatory Services, 11) be homeless, 12) be previously recorded as dropping out, or 13) reside in a residential placement facility, substance abuse treatment facility, emergency shelter, psychiatric hospital, halfway house, or foster home. At-risk students are assigned a value of “1” and non-at-risk students are assigned a value of “0”. This variable comes from the PEIMS data.

Attendance. The variable represents the total number of days a student was present in school during a school year. It is the sum of the six 6-week attendance variables from the PEIMS data.

Economic Disadvantage. This variable indicates if a student is eligible for free/reduced price lunch, or other public assistance. Economically disadvantaged students are assigned a value of “1” and non-economically disadvantaged students are assigned a value of “0”. This variable comes from the PEIMS data.

Gifted and Talented. This is a variable indicating if a student is participating in a state-approved gifted/talented (GT) program. Students participating in a GT program are assigned a value of “1” and students not participating in a GT program are assigned a value of “0”. This variable comes from the PEIMS data.

Limited English Proficient. A student in classified as Limited English Proficient (LEP) if 1) a language other than English is used as the primary language in the home and 2) the student's English language proficiency is determined to be limited by a Language Proficiency Assessment Committee (LPAC) or as indicated by a test of English proficiency. Most students identified as LEP receive bilingual or English as a second language instruction. Students identified as LEP are assigned a value of “1” and non-LEP students are assigned a value of “0”. This variable comes from the PEIMS data.

Mobility. This set of variables indicates the cumulative number of school moves a student has made over their educational careers. This variable was constructed using the school enrollment information from the six 6-week enrollment periods frim the PEIMS data. Three types of mobility were calculated: 1) within-school-year mobility, which occurs when a student transfers to a new school during a school year, 2) between-school-year mobility, which occurs when a student transfers to a new school between school years, and is not in a terminal grade (e.g., 5^{th} grade or 8^{th} grade), and 3) structural mobility, which occurs when a student naturally transfers from elementary to middle school or from middle school to high school.

Race/Ethnicity. Students are recorded as belonging to one of five racial/ethnic groups: American Indian/Alaska Native, Asian/Pacific Islander, Black, Hispanic, or White. This variable comes from the PEIMS data. A series of four dummy variables were constructed, identifying to which racial/ethnic group each student belongs with "white" serving as the reference group. Although American/Indian/Alaskan Native students were included in the analyses, results pertaining to this group of students have been masked to comply with FERPA guidelines on the reporting of research findings when small cells are present.

Sex. The sex of students, recorded as female or male. Female students are assigned a value of "1" and male students are assigned a value of "0". This variable comes from the PEIMS data.

Special Education. This variable identifies students that are participating in special education services. These students have been identified as having at least one disability by an Individualized Education Program (IEP) committee. Students participating in special education services are assigned a value of “1” and students not participating in special education services are assigned a value of “0”. This variable comes from the PEIMS data.

School-level covariates. Since previous research on school closures has demonstrated that schools that close tend to have lower enrollment and achievement than schools that remain open, I also control for these factors in the propensity models.

Enrollment. This variable captures the annual enrollment size (i.e., number of students) of each school in the sample. This variable comes from the AEIS data.

Achievement. The achievement of each campus in the sample is captured by two complimentary variables: the proportion of students annually that score at or above the satisfactory-level on TAKS mathematics and reading. This variable comes from the AEIS data.

Phase II—Dependent Variable

Students’ annual raw TAKS scores in mathematics and reading were used to estimate the effects of closure and reassignment on academic achievement. Annual TAKS scores in mathematics and reading were obtained from the ERC’s TAKS database for the years 2002-03 through 2009-10. The raw scores indicate the number of test questions students answered correctly. As mentioned preciously, scores generally range from 0 to 60, with slight variations in the maximum score across subjects and years.

Phase II—Independent Variables

School Closure. To quantify the effects of closure and reassignment on the academic trajectories of displaced students, the timing of school closures was tracked via two complementary variables. First, to quantify the immediate effects of closure on academic achievement a dichotomous variable was constructed that indicates when a student experienced a closure. Students are assigned a value of "0" in the years prior to experiencing a closure and a value of "1" in the years after they experience a closure. Students never experiencing a closure are assigned a value of "0" for the entire study period. When included in the discontinuous change models, this variable provides an estimate of the average effect of closures on student achievement in the year immediately following a closure.

Second, to estimate the longer-term effects of closures on academic achievement, a continuous variable was constructed that tracks the number of years since a student experienced a closure. Prior to a closure and in the year immediately following a closure, the variable takes on a value of "0", and then increase annually by a value of "1". For instance, suppose a given student's school closed at the end of the 2004-5 school year. If we track the student's progress from 2002-03 to 2009-10, then the variable will take on a value of "0" from 2002-03 (the first school year for which data are available) to 2005-06 (the first school year the student attends their new school after being discalced).

Beginning in 2006-07, however, the variable begins to track the number of years since the student experienced a closure. As such, in 2006-07 the variable will take on a value of “1”, in 2007-08 it will take on a value of “2”, and so on. Again, students never experiencing a closure will receive a value of “0” for the entire study period. When included in the discontinuous change models, this variable provides an estimate of the average change in students’ achievement trajectories over time.

Student and School Characteristics. Although the sample of displaced students were matched to control students along a variety of school- and student-level characteristics, I include a parallel array of predictors in the discontinuous change models. Because these covariates are the same as the covariates used in Phase I of the analysis, I do not discuss them again here.

Phase III—Variables

No additional variables are required for the estimation of the robustness indices.

Sample

Study Period

The state of Texas has frequently shifted its assessment system, with the current State of Texas Assessments of Academic Readiness (STAAR) exams constituting the fifth assessment regime since the establishment of the Texas Assessment of Basic Skills (TABS) in 1980. To ensure the longitudinal comparability of student achievement data across the study period, I limit my analysis to closures that occurred between 2002-03 and 2009-10, during which Texas administered the Texas Assessment of Knowledge and Skills (TAKS).

Identification of Closed Schools

School closures in Houston were identified using two complimentary methods. First, using the AEIS school-level enrollment data, I tracked each school's unique identifier over time. If a school disappeared from the database, it was flagged as a closure. Second, I shared this list of closures with Carla Stevens, Assistant Superintendent of Research and Accountability at HISD. Her department subtracted several schools form this list, based on institutional information that is not publicly available. The schools that were removed from the list of closures were predominantly campuses that closed down temporarily for construction or that were restructured in some way but remained at the same address.

Based on this procedure, I identified over 70 school closures in HISD since 2000, 46 of which were closed during the TAKS era. Of these 46 closures, 34 were elementary, 3 were middle, and 8 were high schools. Together these schools enrolled a total of 11,786 students, accounting for 2.4% of the 500,370 students that were enrolled in HISD between 2003 and 2010.

Identification of Displaced Students

Within the sample of 46 closed schools, I excluded from analysis those students who experienced closures prior to 3rd grade and after 10th grade. Because TAKS tests were not administered to students prior to third grade, I have no baseline data on the achievement of these students that would permit estimation of the impact of closures. Conversely, because students did not participate in TAKS testing after 11th grade, I have no post-closure data that would permit me to estimate the impact of closures on students that experienced closures after 10th grade. After application of these criteria, I retained a sample of 6,855 displaced students.

Prior to matching the sample of 6,855 students to a comparable sample of non-displaced students, I merged the closure indicator identifying the 6,855 displaced students back into the full longitudinal data set. I then imputed missing values on all variables used in Phases I and II of the analysis. I describe the imputation procedure below.

Imputation Procedure

As is the case with school administrative records, students often have missing values on one or more of the variables in the ERC data. To reduce bias and increase efficiency vis-à-vis list-wise deletion methods, I impute missing values on all predictors used in the analyses via the Amelia II package in R. I impute five unique data sets, which are generally deemed sufficient to obtain a valid inference (Rubin, 1987; van Buuren, Boshuizen & Knook, 1999). I also leverage Amelia's time-series capability, adding linear and quadratic terms to the model to improve imputation of missing values (Honaker, King & Blackwell, 2015). For each continuous variable, I set bounds corresponding to the logical minima and maxima (e.g., percentages must range from 0 to 100).

I obtain the final treatment effects by pooling the coefficients and standard errors from these five regression models using Rubin's (2004) rules for combining multiplyimputed data sets. The overall effects for each coefficient in the models is simply the mean of the estimates from the five imputed data sets. These overall effects are given by,

$$\bar{X} = \frac{1}{I}\sum_{i=1}^{D} \hat{X}_i,$$

where $\bar{X}$ is the mean of the coefficients across the five imputed data sets, I is the total number of imputations, and $\hat{X_i}$ is a vector of the five coefficients of a given variable, X. The overall variance, V, of each coefficient is the product of two components: the mean of the squared standard errors within the imputed datasets ($\bar{W}$), and the between-imputation variability in the coefficient estimates (B). The within-imputation variability is given by,

$$\bar{W} = \frac{1}{I}\sum_{i=1}^{D} W_i,$$

where W_i is a vector of the five variances for estimate $\bar{X}$. The between-imputation variability is given by,

$$B = \frac{1}{I-1}\sum_{i=1}^{D}\left(\hat{X}_i - \bar{X}\right)^2.$$

The overall variance, V, associated with $\bar{X}$ is given by,

$$V = \bar{W} + (1 + I^{-1})B,$$

where $(1 + I^{-1})B$ corrects for the additional variability in the estimate due to missing data. A significance test can be constructed from these pooled estimates, testing the null hypothesis that a parameter is equal to zero (i.e., $X = X_0$). This statistic has a *t*-distribution with d degrees of freedom and is given by,

$$t_d = \frac{\bar{X} - X_0}{\sqrt{V}}.$$

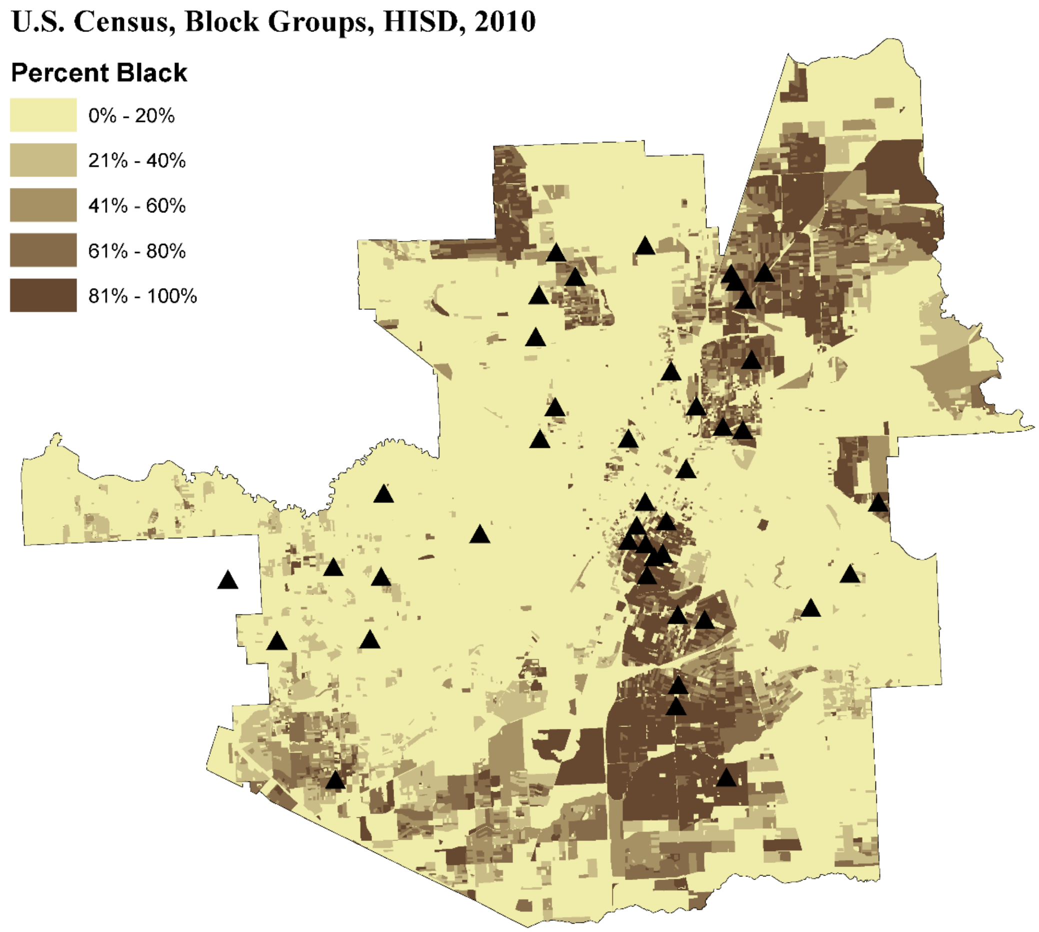
Finally, as Ruben (1987) discussed, the degrees of freedom for the imputed data are different from the degrees of freedom that would be have been used if no data were missing. Specifically, the degrees of freedom must be adjusted downward to account for the additional uncertainty created by the missing data. This estimate of d is given by,

$$\hat{d} = (I - 1)\left[1 + \frac{\bar{W}}{(1+I^{-1})B}\right]^2.$$

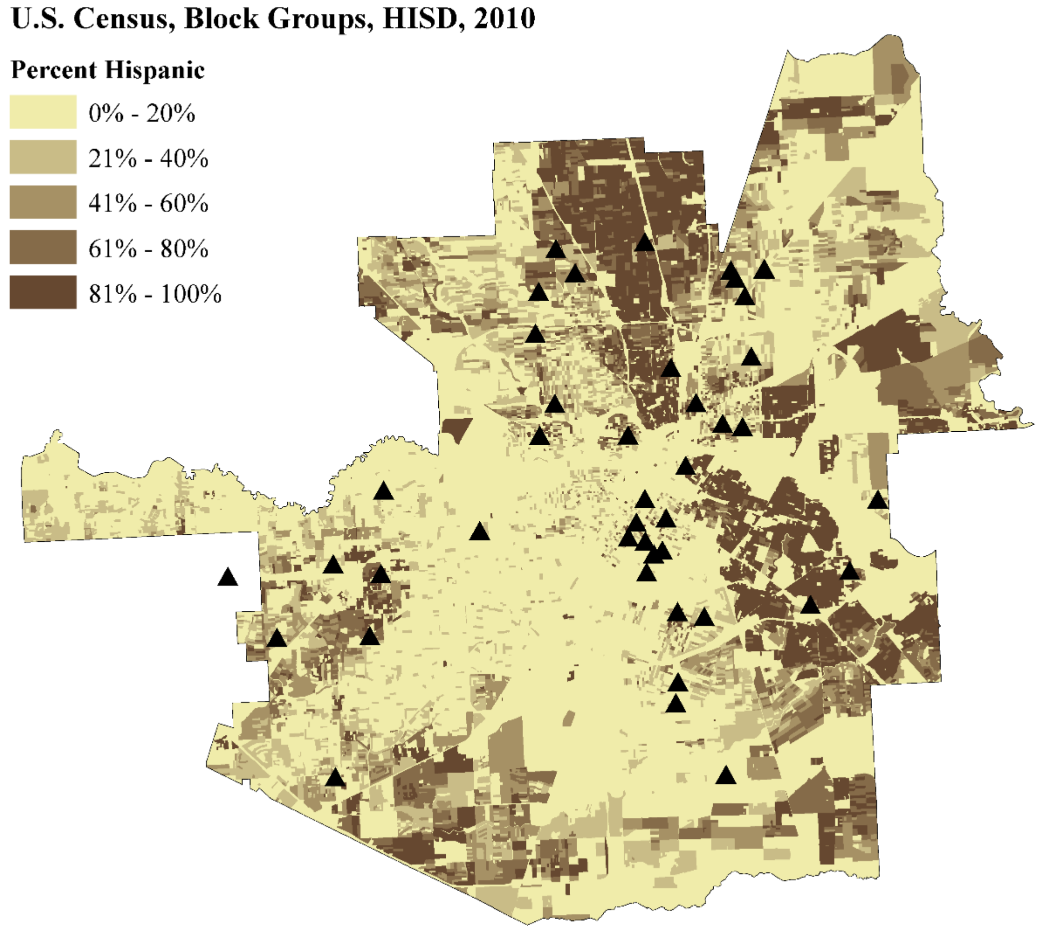
Post-Imputation Sample Descriptives. Consistent with research on closures in other major cities, Table 3 demonstrates that the schools closed in HISD differ systematically from schools that never closed. Unsurprisingly, closed campuses tended to enroll roughly 400 fewer students on average than schools that remained open. Closed schools also tended to be relatively low-performing academically, with pass rates that were 20.6 and, 14.7 percentage points lower than schools that remained open on math, and reading, respectively. While schools that closed were roughly three times as likely to be deemed academically unacceptable as schools that remained open (19% vs. 6%), surprisingly the majority were academically acceptable or better (81%) and more than a quarter (27%) were recognized or exemplary.

In terms of racial/ethnic composition, closed schools were 4 percentage points less white, 9 percentage points less Hispanic, and 15 percentage points more black, on average, than schools that remained open. Closed schools also enrolled 8.6 percentage points more economically disadvantaged students than schools that remained open.

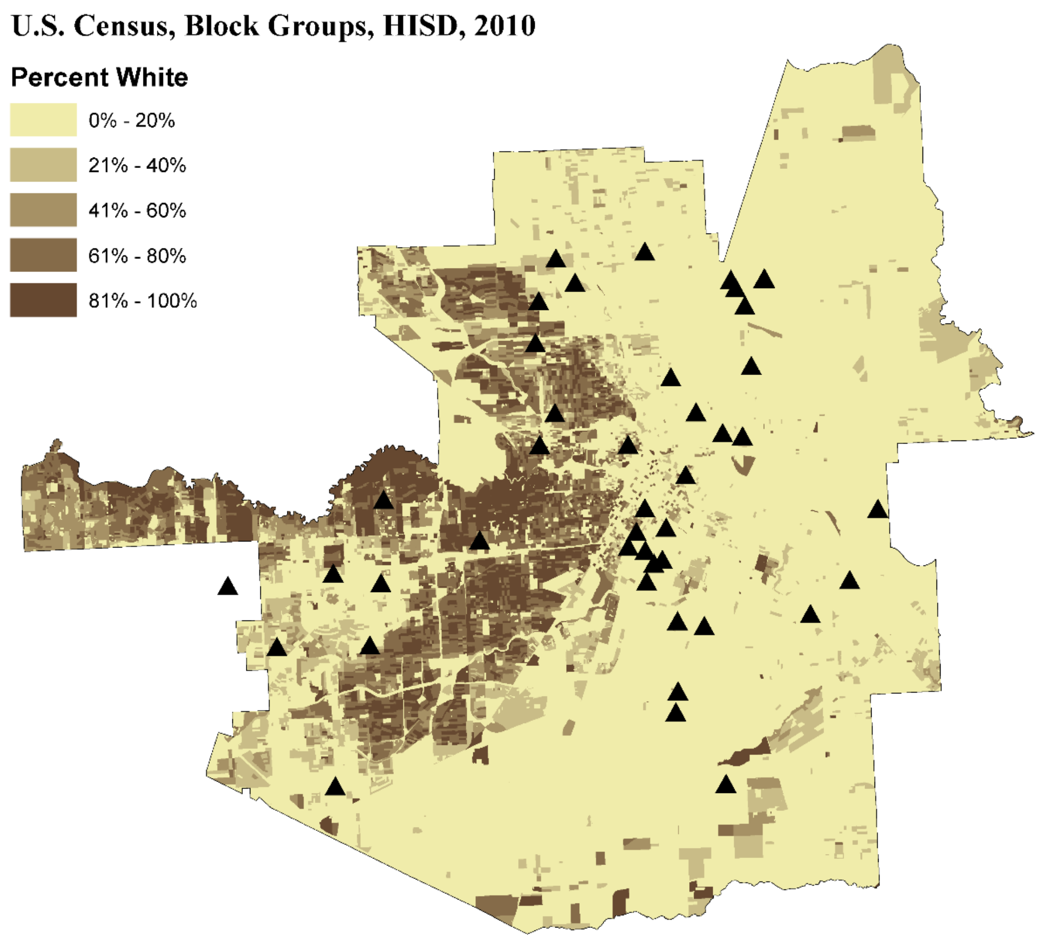
Consistent with these findings, Maps 1 through 3 illustrate the spatial distribution of the 46 school closures that occurred in HISD between 2003 and 2010. Specifically, these three maps show the geographic location of closed schools, relative to where blacks, whites, and Hispanics/Latinos live within the district's boundaries, respectively.



Map 1. Relationship between the residential locations of blacks relative to the 46 school closures that occurred in HISD between 2003 and 2010. The black triangles represent the location of schools that closed. The brown shading represents the proportion of individuals within each Census block that identify as non-Hispanic black. Census block boundaries have been removed to enhance visual clarity.



Map 2. Relationship between the residential locations of Hispanics relative to the 46 school closures that occurred in HISD between 2003 and 2010. The black triangles represent the location of schools that closed. The brown shading represents the proportion of individuals within each Census block that identify as Hispanic. Census block boundaries have been removed to enhance visual clarity.



Map 3. Relationship between the residential locations of whites relative to the 46 school closures that occurred in HISD between 2003 and 2010. The black triangles represent the location of schools that closed. The brown shading represents the proportion of individuals within each Census block that identify as non-Hispanic white. Census block boundaries have been removed to enhance visual clarity.

Propensity Score Matching Procedure

The foregoing illustrates that the schools closed by HISD differ systematically from schools that did not close in terms of student achievement and demographics. This decidedly non-random assignment of the closure “treatment” suggests that ordinary least squares (OLS) comparisons of the achievement trajectories of displaced vs. non-displaced students may systematically bias estimates of the causal impact of closures. To minimize this bias, I employ a propensity-score matching procedure to pair displaced students with an appropriate control group of non-displaced students that are similar on an array of observed student and school characteristics.

First, to account for possible cohort effects, I exactly match displaced students to control students in terms of year and grade level. For example, a 4^{th} grader that was displaced by a school closure in 2007-08 is matched to a non-discalced student that was also in 4^{th} grade in 2007-08. I then employ a nearest-neighbor algorithm to match displaced and control students on student- and school-level predictors. At the student level, I match students on: basic demographics (e.g., gender, race/ethnicity, economic disadvantage, age, grade), special program enrollment (e.g., limited-English proficiency, special education), attendance rates, mobility (structural, non-structural), and prior achievement (i.e., reading and math scores). Certain student-level predictors, such as bilingual education enrollment and vocational education enrollment, were excluded from the propensity models owing to collinearity with other variables or to extreme sparseness in the data. At the school level, I match displaced students to control students on: school achievement (i.e., math and reading pass rates), school enrollment, and school grade level.

Consistent with other work, I limit the nearest-neighbor matching algorithm to ensure that all matches fall within a specified caliper distance of 0.25 standard deviations (Rosenbaum & Rubin, 1985; Stuart, 2010). If a treatment student does not have any matched control students within the specified caliper distance, that student is discarded from the matched sample. Students are matched without replacement; as such, each student in the treatment group is uniquely matched to a student in the control group.

As a result of this procedure, across all five imputed pooled samples, I find matches for 6,826 of the 6,855 eligible displaced students (99.6%). This match rate suggests a robust region of common support, with findings generalizable to the great majority of displaced students within HISD.

Sample Balance. I employ several complementary approaches to assess the balance of the treatment and control groups resulting from the propensity score matching procedure. First, I consider the distribution of propensity scores in the treatment and control groups graphically and numerically. Figure 1 provides an example of a histogram of the distribution of propensity scores before and after matching for one of the five imputed data sets. The figure clearly demonstrates that the original sample of unmatched control students are a poor match for the students in closed schools (histograms in the left column). However, because of the propensity score matching procedure, the propensity score distributions of matched treatment and control students are quite similar (histograms in the right column).

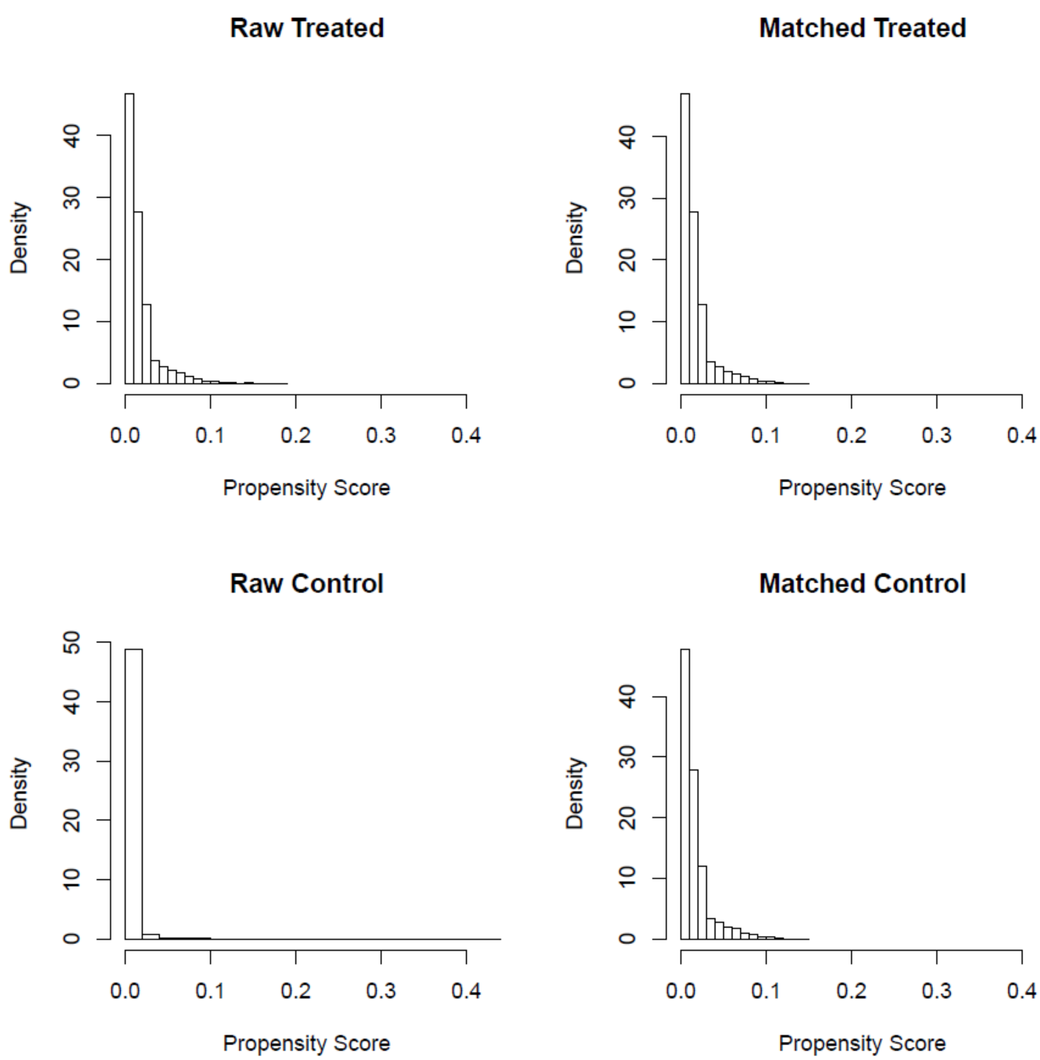


Figure 1. Frequency distributions of students in treatment (i.e., displaced) and control groups (top and bottom, respectively), before and after matching (left and right, respectively).

This difference may be quantified mathematically in terms of the absolute standardized difference in the mean propensity score between the two matched groups (Rubin, 2001), which should be near zero (Stuart, 2010). As Table 4 reveals, after matching, the absolute standardized mean difference in propensity scores of the treatment and control groups is 0.023, well under Rubin's recommended threshold of 0.25. Given the initial average separation between the sample of displaced students and the unmatched comparison sample of non-displaced students, this constitutes an improvement in balance, or a reduction in bias, of 98.1%. In addition, I examine the ratio of the variances of the propensity scores in the matched treatment and control group, which Rubin has suggested should range from 0.5 to 2.0 (2001). The ratio of the variances of the two groups is 0.99 across all imputed matched data sets, suggesting that the variance, as well as the central tendency, of the propensity scores of treated students are well matched to that of control students.

Finally, following the procedure of Rosenbaum & Rubin (1985), I compare the level of balance before and after matching as well as the percent reduction in bias on each individual covariate. Prior to matching, the standardized mean difference between the treatment and control groups is generally problematic in that they exceed 0.2 (20%). However, after matching, as Figure 2 illustrates, all covariates exhibited a degree of bias of less than 20%. Bias was reduced for the great majority of covariates, with slight increases in bias evident only for a handful of variables with very low degrees of initial bias (e.g., gender, special education enrollment).

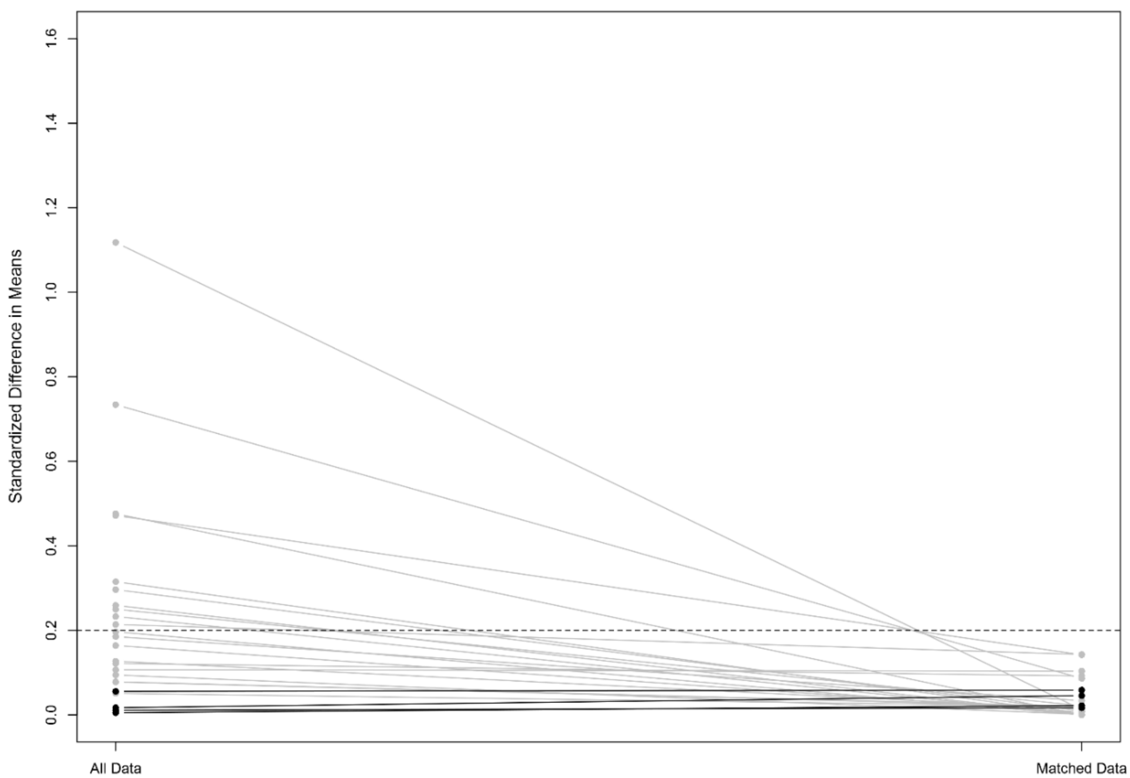


Figure 2. Absolute standardized difference in covariate means between treatment and control groups for all vs. matched data.

Taken together, balance analyses suggest that the matching procedure yielded a well-matched sample of control students that are highly similar to the treatment group of displaced students in terms of a broad range of observed predictors. The final analytic sample used in Phase II of the analysis consists of 13,652 students (6,826 displaced students, and 6,826 non-displaced students).

Analytic Strategy

Estimating the effect of Closures on Student Achievement

To isolate the effect of school closures, I estimate a series of multilevel longitudinal discontinuous change models (Singer & Willett, 2003) predicting student achievement as a function of school closure. I compare the trajectories of students displaced by closure to the counterfactual trajectories of the propensity score-matched sample of control students that did not experience a closure. The models take the general form:

$$\begin{aligned} Y_{tij} = \alpha_j &+ \beta_1(Year_{tij}) + \beta_2(Year_{tij})^2 + \beta_3(Close_{tij}) + \beta_4(PostClose_{tij}) \\ &+ \beta_5(PostClose_{tij})^2 + \beta_a(Stu_{ij}) + \beta_b(Stu_{tij}) + \beta_c(Sch_{ij}) \\ &+ \beta_d(Sch_{tij}) + \beta_e(Sch_{tij} \times Close_{tij}) + \beta_f(Sch_{tij} \times PostClose_{tij}) \\ &+ \varepsilon_{tij} + \mu_{ij} + \delta_j \end{aligned}$$

Where Y represents the achievement on the TAKS assessment in math or reading (as measure via raw TAKS scores) in year t of student i in school j. Counterfactual linear and curvilinear change in student achievement over time are captured via β_1 and β_2, respectively.

The effects of closure on achievement are estimated for the year immediately following a closure as well as for the slope of achievement over time (β_3 and β_4, respectively). In addition, because the impact of closures may be larger in initial years and taper off over time (or vice versa), I use a quadratic term (β_5) to capture the curvilinearity in the impact of closures. Tests of model fit suggested that models incorporating quadratic effects of closure substantially improved the fit of the data beyond linear models.

Although the sample of displaced students were matched to control students along a variety of school- and student-level characteristics, I provide doubly-robust treatment estimates by including a number of student-level and school-level covariates in the regression models (Stuart, 2010), a technique which Shadish, Clark, and Steiner (2008) have found improves bias reduction vis-à-vis matching alone. These doubly-robust estimates are represented in the model above as β_a (time-invariant student-level), β_b (time-varying student-level), β_c (time-invariant school-level), and β_d (time-varying school-level). All continuous covariates are grand-mean centered prior to inclusion in the regression models. Finally, in lieu of school-level covariates, I also specify alternative models using school fixed effects to account for unobserved heterogeneity between schools.

In addition to the propensity-score matched models, I also estimate a series of parallel ordinary least squares (OLS) models comparing the achievement trajectories of students displaced by closure to those of all students not displaced by closure. Estimated these OLS models alongside the propensity-match models allows me to compare my findings to those that would have been obtained using less robust, descriptive methods.

Finally, to assess how the quality of receiving schools moderates the impact of school closures on student achievement, I also incorporate interaction terms between the closure variables and receiving school performance in the set of models containing school-level variables without school fixed effects (β_e and β_f).

Robustness Checks

Although I use propensity matching techniques to account for the systematic differences between students that experience closures and students that do not experience closures, there is no way to determine with certainty that the effects of non-random treatment assignment have been eliminated completely. Indeed, one limitation of propensity score techniques is that they can only account for measured sources of treatment bias. If, however, the propensity models have been specified incorrectly because an important predictor of displacement by closure has been omitted or is simply not measurable, the analytic approach described above will be biased.

To address this limitation, the casual estimates derived from Phases II of this study are supplemented with robustness indices according to the procedures of Frank and colleagues (Frank, 2000; Frank, Maroulis, Duong, & Kelcey, 2013). Following this technique, I quantify the robustness of my estimates regarding the impact of closures on student achievement in terms of the omitted variable bias that would need to be present to invalidate my causal inferences. Specifically, I estimate the minimum proportion of cases (i.e., students) in my sample that would need to be replaced with counterfactual cases (i.e., cases in which school closure had zero effect on achievement) to cause my estimates of the closure effect to become statistically non-significant. The robustness indices are given by,

$$\% \, Bias \ necessary \ to \ invalidate \ inference = \frac{1-\delta^{\#}}{\hat{\delta}},$$

where, $\delta^{\#}$ is the critical threshold from a t-distribution, and $\hat{\delta}$ is the estimated effect of interest. All robustness indices were computed using the KonFound-it! application (Frank, 2014).

Although robustness indices cannot determine if an important variable has been omitted from an analysis, they do provide important information regarding how much confidence can be placed in a causal inference. For instance, consider two hypothetical analyses. The causal inference made in the first analysis has a robustness index of 0.9, while the second has an index of 0.1. This suggests that nearly all the cases from the fist study (90%) would need to be replaces with null cases before the causal inference could be invalidated. In the second study, however, hardly any of the cases (10%) would need to be replaced with null cases before the inference was rendered invalid. These findings suggest that the first study is far more robust against the threat of potential omitted variable bias than the second.

CHAPTER 4: RESULTS

Overview

In this chapter, I present my findings for the three primary research questions of this study. To address the first research question regarding the overall impact of closures on the achievement trajectories of displaced students, I present the results of from a series of multi-level, longitudinal discontinuous changes models, estimating the immediate and longer-term impact of closures on the achievement of discalced students. To address the second research question, I add interaction terms to these models estimating the moderating effect of student race/ethnicity and economic disadvantage status, respectively, on the relationship between school closure and student achievement. To address the third research question, I add interaction terms to models above, estimating the moderating effect of receiving school performance on the relationship between school closure and student achievement. Finally, to address the fourth research question regarding systematic differences in receiving school quality by student characteristics, I compute the probability of a displaced student transferring to a school of a given level of quality disaggregated by a variety of student characteristics.

Research Question 1: How do Closures Effect the Short- and Longer-term Achievement of Displaced Students in HISD?

Tables 5 and 6 presents the results of multilevel longitudinal models estimating the impact of school closures on the level and slope of student academic achievement in math and reading, respectively. The tables present estimates for two classes of models: 1) ordinary least squares (OLS) models comparing all students that experienced closures to all students that did not experience closures (Table 5, Model 2 and Table, Model 6); and 2) propensity score matched models comparing displaced students to comparable control students that did not experience closures (Table 5, Models 2-4 and Table 6, Models 6-8). For the matched sample, I present three estimates of the closure effect: 1) main effect estimates of the closure effect without student- or school-level controls (Table 5, Model 2 and Table 6, Model 6); 2) doubly-robust estimates incorporating student- and school-level covariates (Table 5, Model 3 and Table 6, Model 7), and 3) doubly-robust estimates including student-level covariates and school fixed effects (Table 5, Model 4 and Table 6, Model 4). Because the propensity score models with school fixed effects incorporate sources of unobserved as well as observed school heterogeneity in student achievement, I focus on these estimates, which should provide the most unbiased estimate of the effects of closure on student achievement.

Math Achievement

Across all model specifications, I find that closures are consistently associated with a positive "bump" in math achievement in the year immediately following closure, although the magnitude and statistical significance of this estimate varies somewhat across models.

As Figure 3 illustrates, displaced students score on average 0.70 points higher on math TAKS in the year following a closure than their counterparts in schools that remained open. As the graph illustrates, for example, a student displaced after 4th grade, between 2004 and 2005, scored an estimated 0.09 standard deviations higher on their 2005 math TAKS than a non-displaced student (28.77 vs. 28.08). While the multilevel OLS models also reveal a positive effect of closures on match achievement, I find that these models underestimate the magnitude of the positive impact of closures vis-à-vis more robust models (0.06 SDs).

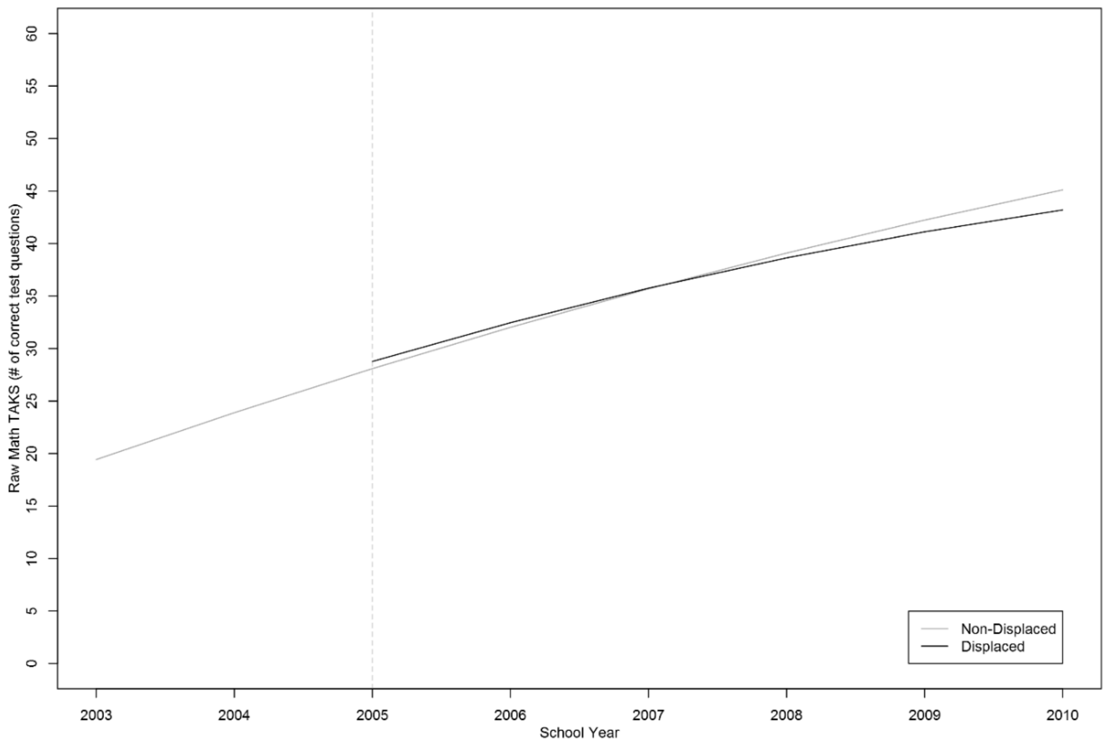


Figure 3. Estimated main effect of school closure on displaced students' math achievement. Effect of a school closure on a fourth grader displaced after the 2003-04 school year. The vertical dashed gray line represents the first year the student attended their new school after being displaced. The solid grey line represents the counterfactual math trajectory of displaced students had they not experienced a closure. The solid black line represents the estimated math achievement of students experiencing a closure.

Results of the impact of closures on math achievement slopes post-closure are somewhat less encouraging. Indeed, displaced students experience flatter growth trajectories after closure than non-displaced students, thus progressively eroding the short-term positive effects of closure on math achievement. For example, the immediate gain in 5th grade math achievement experienced by a student displaced after 4th grade, between 2004 and 2005, is virtually eliminated by 7th grade. By 10th grade, in 2010, the displaced student scores an estimated 0.17 standard deviations lower than their non-displaced peers (43.20 vs. 45.11).

Even more troubling, displaced students fall farther and farther behind their non-displaced peers with each passing year, as the gap in the math achievement growth between displaced and non-displaced students worsens over time. As Figure 3 illustrates, the growth in achievement between the first and second years after closure for a student displaced after 4th grade is 93.8% as large as that of their non-displaced peers (2005 to 2006 change of 3.69 vs. 3.93); however, by the sixth year after closure, the same student experienced just 72.4% of the growth in achievement of their non-displaced peers (2009 to 2010 change of 2.08 vs. 2.88).

Reading Achievement

Analyses reveal that the pattern of effects of closure on reading achievement differs from that of math achievement. As Figure 4 illustrates, I do not observe a comparable “bump” in reading achievement after closure mirroring that observed for math achievement. Although the immediate effect of closure is generally negative across the propensity-score matched models, the doubly-robust models reveal that the effect is not statistically significant after controlling for student and school characteristics.

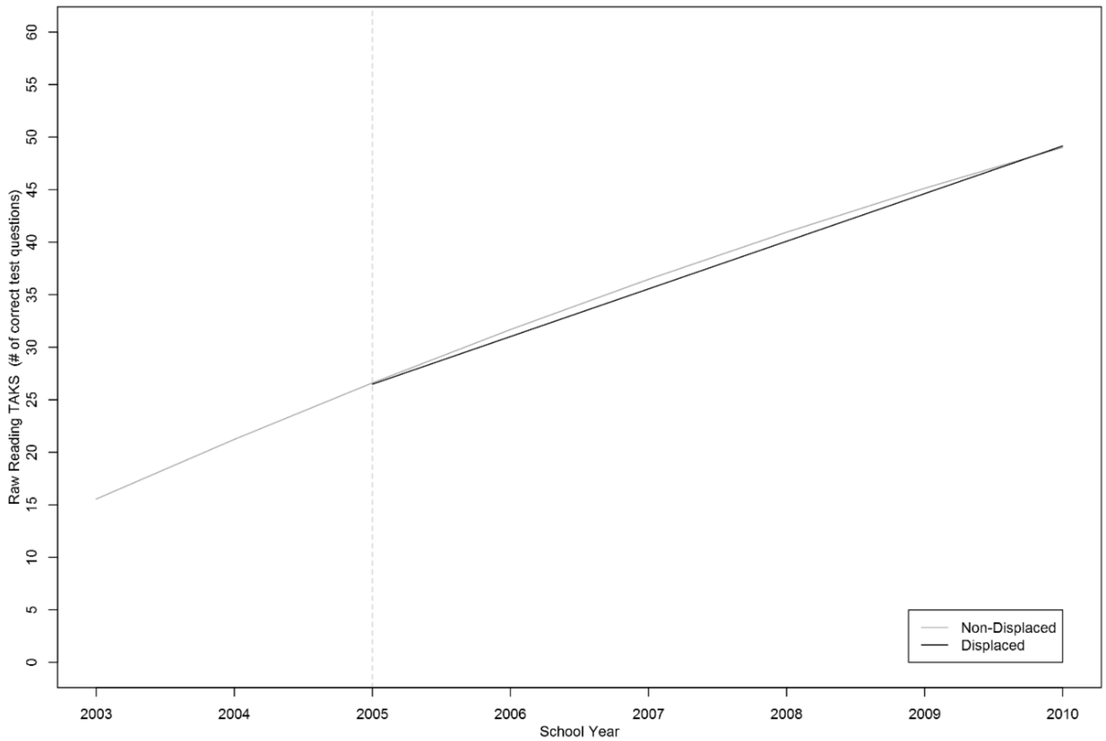


Figure 4. Estimated main effect of school closure on displaced students' reading achievement. Effect of a school closure on a fourth grader displaced after the 2003-04 school year. The vertical dashed gray line represents the first year the student attended their new school after being displaced. The solid grey line represents the counterfactual reading trajectory of displaced students had they not experienced a closure. The solid black line represents the estimated reading achievement of students experiencing a closure.

In the longer-term, I observe a convex curvilinear effect of closures on student reading achievement. Students that experience closures initially have slower growth trajectories in the years after closure than non-displaced students. Unlike math achievement, however, I find that displaced students progressively "catch up" to their non-displaced peers as their achievement slopes increase at a faster rate than those of their non-displaced peers. For example, the growth in achievement between the first and second years after closure for a student displaced after 4^{th} grade is 89.3% as large as that of their non-displaced peers (2005 to 2006 change of 4.54 vs. 5.08, respectively); however, by the sixth year after closure, this student experienced 116.7% of the growth in achievement of their non-displaced peers (2009 to 2010 change of 4.54 vs. 3.89, respectively). As a result, by 2010, this student has a comparable level of reading achievement as their non-displaced peers (49.15 vs. 49.11).

Robustness Checks

I quantify the robustness of the estimates regarding the impact of closures on student achievement in terms of the bias that would need to be present to invalidate the inferences from the analyses discussed above. These robustness checks provide additional support for my casual inferences regarding the impact of closures on math achievement. Again, I focus on the results of the propensity score models with school fixed effects, which should provide the most unbiased estimate of the effects of closure on student achievement.

Specifically, regarding the short-term positive effect of closures on the level of math achievement, I find that nearly three-fifths (58%) of my cases would need to be replaced with cases for which there is a closure effect of zero to invalidate the observed positive effect. As with the immediate impact of closures on math achievement, the negative curvilinear effect of closures on the slope of math achievement is robust to threats to causal inference: 56% of cases would need to be replaced with cases for which there is an effect of zero to invalidate the observed effect.

Although I did not detect a statistically significant effect of closures on reading achievement in the year immediately following closure, supplemental analyses reveal that the curvilinear effect of closures on the slope of reading achievement is robust to threats to causal inference: roughly three-fifths of cases would need to be replaced with cases for which there is an effect of zero to invalidate the observed effects (62% for linear term, 58% for quadratic term).

Research Question 2: How do the Effects of Closures vary by the Race/Ethnicity and Socioeconomic Status of Displaced Students?

To assess how the impact of school closures varies by the race/ethnicity and socioeconomic status of displaced students, I estimate two sets of models incorporating interaction terms into the models in Tables 5 and 6. First I incorporate the interaction between the closure treatment effect and student race/ethnicity into the two-level propensity-matched, school fixed-effect models from Tables 5 (Model 4) and 6 (Model 8). Second, I incorporate the interaction between the closure treatment effect and student economic disadvantage into the two-level propensity-matched, school fixed-effect models.

Table 7 and 8 presents the results of these interaction models for race/ethnicity and economic disadvantage, respectively. Since none of the school closure by race/ethnicity interactions are significant I do not graph the results from these models. Similarly, I do not graph the results from the model predicting reading achievement as a function of a school closure by economic disadvantage interaction because the results are not significant. To aid in the interpretation of the significant interaction between school closure and economic disadvantage, Figures 5 and 6 graphically depict the impact of closures on the math achievement of comparable economically disadvantaged and non-disadvantaged students, respectively.

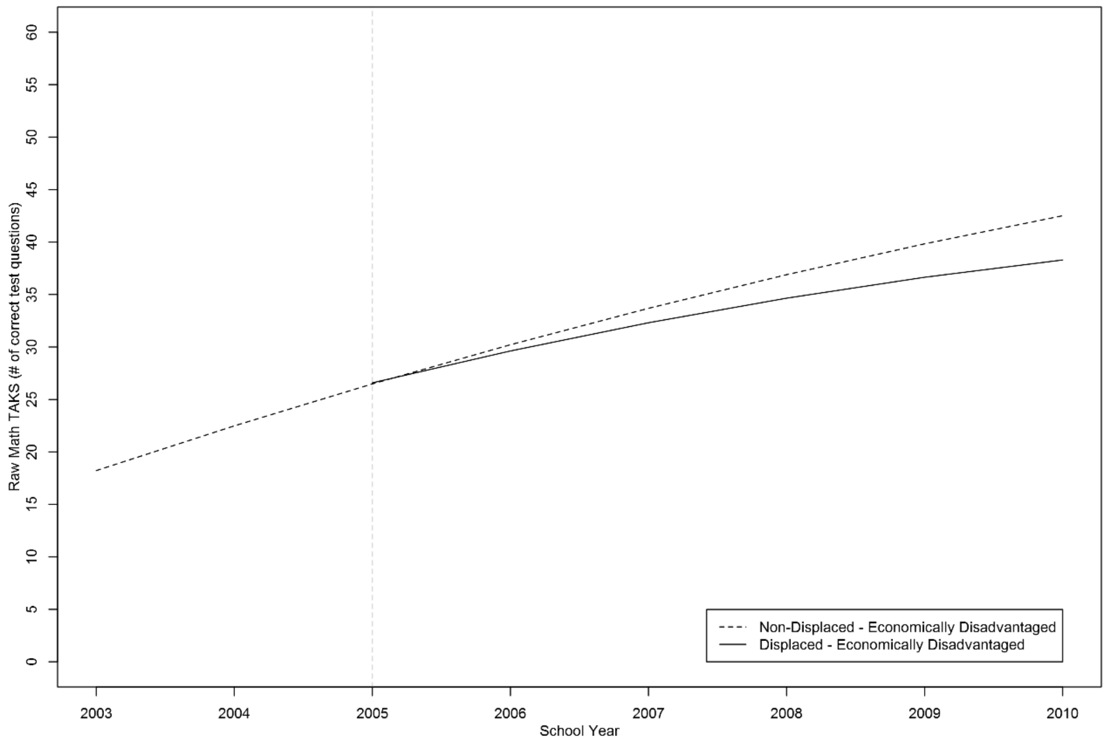


Figure 5. Estimated effect of the interaction between school closure and the socioeconomic status of displaced students on math achievement. This figure depicts the effect of closures on only economically disadvantaged students. Effect of a school closure on a fourth grader displaced after the 2003-04 school year controlling for the performance of the receiving school.

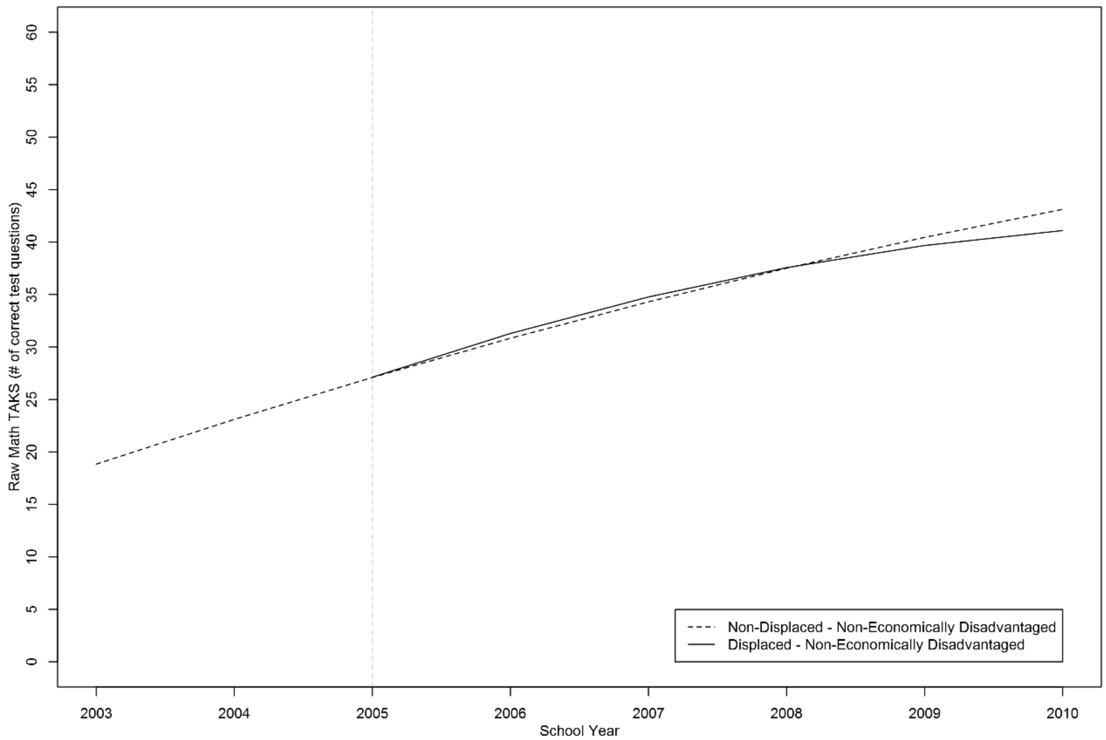


Figure 6. Estimated effect of the interaction between school closure and the socioeconomic status of displaced students on math achievement. This figure depicts the effect of closures on only non-economically disadvantaged students. Effect of a school closure on a fourth grader displaced after the 2003-04 school year controlling for the performance of the receiving school.

Math Achievement

Results from the model incorporating the school closure by student economic disadvantage interaction, reported in Table 8 (Model 11) and depicted in Figures 5 and 6, demonstrate that the effect of closures on the slope of math achievement depends on the economic disadvantage status of displaced students. Closures do not appear to moderate the immediate impact of closures on student math achievement.

Troublingly, Figures 5 and 6 reveal that displaced economically disadvantaged students tend to fall behind their non-disadvantaged peers over time. Indeed, despite having comparable levels of math achievement in the year immediately following closures, the math achievement trajectories of disadvantaged students are significantly flatter than those of comparable non-disadvantaged students that are displaced by closures. Indeed, by 3 years after closure, disadvantaged students score, on average 2.6 points lower on the math TAKS than comparable non-disadvantaged students. Although the widening of the gap between disadvantaged and non-disadvantaged students appears to slow over time, by 6 years after closure, when the displaced students are in 10th grade, the gap has increased to 2.8 points on average.

Reading Achievement

Findings reveal a dissimilar pattern of results for reading achievement. Neither the race/ethnicity, or the socioeconomic status of displaced students moderated the relationship between school closures and reading achievement.

Research Question 3: How Is the Effect of Closures on Achievement Related to the Academic Performance of the Receiving Schools to Which Displaced Students Transfer?

To assess how the impact of school closures varies by the academic quality of the schools to which students transfer, I estimate models incorporating interaction terms to the models in Tables 5 and 6. Because I interact the closure treatment effects with a measure of school performance, quantified in terms of the percentage of students meeting the passing standard for TAKS in math or reading, I estimate three-level propensity-score matched models with student- and school-level covariates, and exclude school fixed-effects. Table 9 presents the results of these interaction models for both math and reading. To aid in interpretation of these effects, Figure 7 graphically depicts the impact of closures on math achievement from the propensity-score matched interaction model (See Table 9).

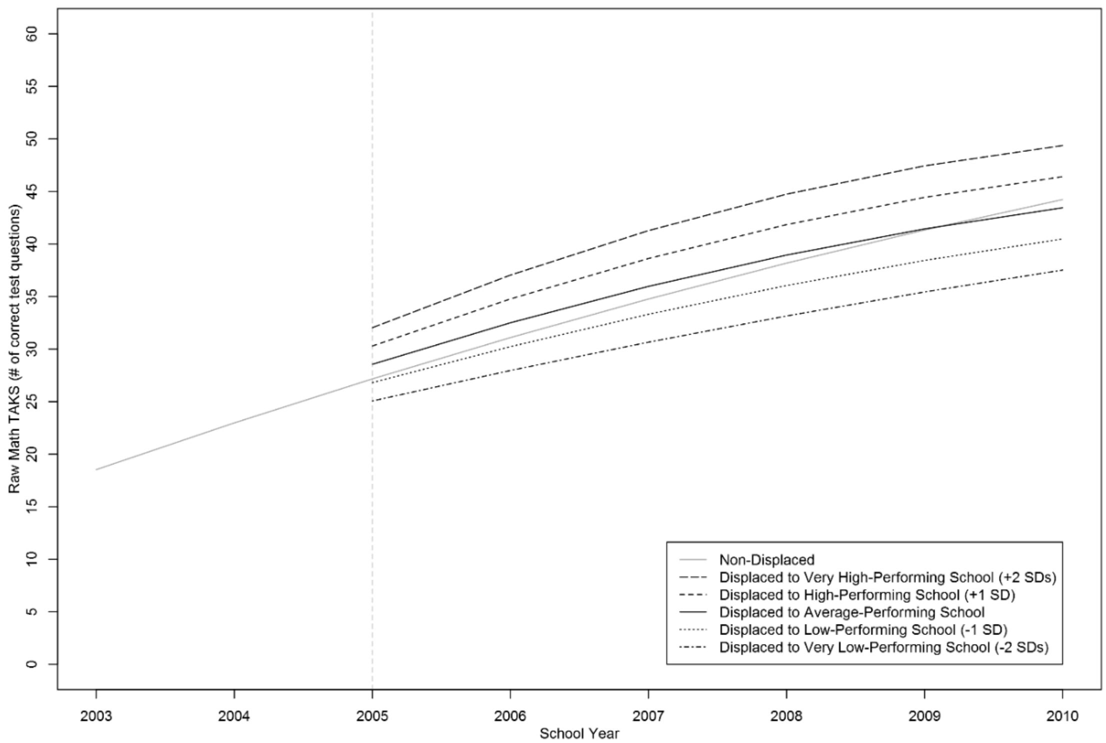


Figure 7. Estimated effect of the interaction between school closure and receiving school performance on displaced students' math achievement. Effect of a school closure on a fourth grader displaced after the 2003-04 school year who transferred to very high-, high-, average-, low- and very low-performing schools. Receiving school performance computed as the proportion of students at a school that met or exceeded the state's accountability standard.

Math Achievement

Results of the propensity score-matched model, reported in Table 9 and depicted in Figure 7, demonstrate that the effect of closures on both the intercept and slope of math achievement is dependent on the academic quality of the receiving school to which displaced students transfer.

I find that the initial “bump” in math achievement observed in the main effect models above (Table 6, Model 6) is driven by displaced students that transferred to moderate- to high-achieving schools. For example, a student experiencing a closure after 4th grade between 2004 and 2005 that transferred to a school with a math pass rate one standard deviation above the HISD average (82%) has an expected 5th grade math TAKS score that is 0.42 standard deviations higher than a comparable student that did not experience a closure (30.294 vs. 27.161). Conversely, students that transferred to substantially lower-performing schools exhibited comparable or slightly worse than expected math achievement in the year following closure than their non-displaced peers. For example, a comparable student transferring to a school one standard deviation below the HISD average (44%) has an expected 5th grade math score that is 0.05 standard deviations below their non-displaced peers. Moreover, a much larger proportion of displaced student transferred to high-performing schools than low-performing schools (20.7% vs. 7.5%).

Overall, a student experiencing a closure after 4th grade between 2004 and 2005 need only transfer to an elementary school with a math pass rate greater than 47.9% (0.8 SDs *below* the HISD average for schools of a given level) to produce a level of post-closure achievement in 2005 that is higher than that of their non-displaced peers. Roughly 90.5% of displaced students transferred to schools that exceeded this threshold.

The figure also reveals that students that transfer to higher-performing schools generally have steeper achievement slopes after closure than students transferring to moderate- and lower-performing schools. Interestingly, however, the slope of displaced students' math achievement after closure is generally flatter than that of non-displaced students, regardless of the quality of school to which they transfer. For students transferring to low- to average-performing schools, the year-to-year growth of displaced students is consistently lower than that of non-displaced students, and widens over time. Students that transfer to higher-performing schools initially have achievement slopes comparable to their non-displaced peers; however, by three years after closure, their achievement slopes are flatter than those of their non-displaced peers.

As a cumulative result of these effects, only students that transferred to substantially higher-performing schools had levels of long-term achievement that exceeded those of their non-displaced peers. Specifically, a student displaced after 4th grade must transfer to an elementary school with a math pass rate greater than 68.2% (0.27 SDs above the HISD average) to produce a level of 10th grade achievement that exceeds that of their non-displaced peers. In the year immediately following closure, just 37.4% of displaced students transferred to a school of sufficiently high quality to produce an achievement over time of this magnitude.

Reading Achievement

Again, findings reveal a dissimilar pattern of results for reading achievement. Findings reveal that the impact of closures on student reading achievement is not associated with the academic quality of the receiving school to which displaced students transfer.

Research Question 4: How does the Academic Performance of Receiving Schools to which displaced students transfer Vary by Student Characteristics?

Table 10 reports the probability of a displaced student transferring to a school of a given level of quality by an array of student characteristics. Because school quality was not a significant moderator of the relationship between school closures and reading achievement, results for math achievement are presented below. The table reveals pronounced differences in the academic performance of schools to which displaced students transfer, particularly in terms of student race/ethnicity and prior achievement.

Over 92% of displaced Asian, Hispanic and White students transferred to schools of sufficiently high quality to produce a level of math achievement in the year following closure that is higher than that of their non-displaced peers. However, just 85.6% of Black students transferred to schools meeting this quality threshold. Not surprisingly, students identified as gifted and talented and students meeting TAKS math passing criteria were also significantly more likely to transfer to schools with levels of achievement necessary to produce increases in math achievement vis-à-vis lower achieving students ($p=0.965$ vs. 0.899 and $p=0.943$ vs. 0.873, respectively); conversely, students enrolled in special education were significantly less likely to transfer to schools that met this achievement threshold than their non-special education peers ($p=0.888$). Interestingly, economically disadvantaged students were slightly more likely to transfer to schools with achievement necessary to produce increases in math achievement. However, it is important to note that the great majority of students in HISD, whether they experienced a closure or not, are categorized as economically disadvantaged. Indeed, in 2009-10, 81% of all students in HISD were list as economically disadvantaged (Author Calculation, NCES CCD, 2010).

These gaps are worsened when examining the characteristics of students that transferred to schools of sufficiently high quality to counteract the negative long-term effects of closure on math achievement over time. Indeed, while just 30.4% of Hispanic students and 31.6% of Black students displaced by closure transferred to schools of sufficiently high quality to produce a level of math achievement six years after closure that is higher than that of their non-displaced peers, 49.2% of White students and 67.1% of Asian students transferred to schools meeting this quality threshold. Economically disadvantaged status was unrelated to the probability of transferring to a school of sufficiently high quality to counteract the negative long-term effects of closure.

Perhaps not surprisingly, given the importance of school quality as a moderator of the closure-achievement relationship and the clear disparities in the quality of receiving schools by student race/ethnicity, I find that black and Hispanic students in the sample that experienced a closure after 4th grade between 2004 and 2005 performed 2.5 and 3.0 points worse on the math TAKS than black and Hispanic students that did not experience a closure by 10th grade. Conversely, displaced Asian students performed 9 points better than their non-displaced peers, while displaced and non-displaced white students had roughly equivalent achievement after six years.

CHAPTER 5: DISCUSSION

Overview

Mirroring the ways in which school closure policy has been implemented in school districts across the country, extant research on school closures has largely ignored the potential disparate impact that closures might have on students from historically disadvantaged populations. In this study, however, I focus attention on the role that closure reform plays in exacerbating educational inequalities across racial/ethnic and socioeconomic boundaries. Overall, I find that the failure of policymakers in Houston to account for the potential uneven impact of closure reform resulted in the districts economically disadvantaged students and students of color shouldering an undue share of the burden of school closures. Not only were poor and non-white neighborhoods disproportionately stripped of their neighborhood schools, but poor and non-white children were disproportionately transferred into receiving schools that were systematically lower-performing than their more advantaged peers.

In this study, I contribute to the emerging picture of the impact of urban school closures, providing evidence of the short- and longer-term effects of closures on student academic achievement, paying attention to the differential impact that closures have on students from different racial/ethnic and socioeconomic backgrounds. Towards that end, this study makes several contributions the extant literature on school closures.

First, while most previous research has been limited to declining urban centers in the Northeast and Midwest, I investigate the impact of closures in the rapidly expanding

Houston metropolitan area, whose central urban district has closed over 70 schools since 2000, despite scant media attention. Second, while extant research has generally concluded that closures have, at worst, a transitory negative impact on student achievement (Brummet, 2014), and at best, a positive impact on student achievement over time (Carlson & Lavertu, 2015), my findings provide a more pessimistic view of the long-term impact of school closures, as short-term increases in achievement are eroded by flatter learning slopes thereafter. Cumulatively, students displaced by closures in elementary school perform nearly one-fifth of a standard deviation below their non-displaced peers in math six years post-closure, depending on the year and grade in which they experienced a closure.

Like previous studies (Brummet, 2014; Carlson & Lavertu, 2015), however, I also find that closures can be beneficial to displaced students if they transfer to schools of sufficiently high quality. Unfortunately, very few students in HISD transfer to schools of sufficiently high quality to result in long-term improvements in achievement. Moreover, even students attending schools with levels of performance two standard deviations above the mean have flatter achievement slopes than those of their non-displaced peers.

Finally, and perhaps most troublingly, the results suggest that students of color, disadvantaged students, and low-achieving students are particularly unlikely to transfer to high-achieving schools, thus exacerbating already troubling achievement gaps. Moreover, analyses suggest that economically disadvantaged students are particularly sensitive to being displaced by school closure. As such, Houston's most disadvantaged students disproportionately bear the negative effects of closure.

The finding that most displaced students experience a short-term "bump" in math achievement in the year following a closure is somewhat surprising, though consistent with the findings of Carlson and Lavertu (2015). While my analyses do not permit me to draw any firm conclusions regarding the cause of this increase, it is plausible that receiving schools targeted displaced students for additional or remedial math instruction. For example, HISD allows its schools considerable flexibility to offer "double blocked" classes for math, as well as tutorial periods for math instruction, as well as "high dosage" tutoring for students performing at or below expected achievement levels (HISD, 2014). Unfortunately, the finding that short-term gains in math achievement are eroded over time is consistent with previous research finding that the short-term gains of such intensive "double-dosing" approaches tend to wear off quickly (Taylor, 2014).

That displaced students generally experience slower math achievement trajectories after closure than their non-displaced peers is also somewhat surprising in light of prior research, which has consistently found steeper growth trajectories in math post-closure (Carlson & Lavertu, 2015; de la Torre & Gwynne, 2009). It is possible that this may be a function of displaced students in HISD transferring to lower-quality receiving schools than in the states and districts studied by other scholars. However, it should be noted that, unlike previous waves of closures in Chicago (de la Torre & Gwynne, 2009), but like findings in Ohio (Carlson & Lavertu, 2015), displaced students in Houston transferred to schools that were significantly higher-performing on average in terms of both math and reading. Alternately, it is possible that while other districts closed only their lowest-achieving schools, HISD closed schools that were somewhat higher-performing relative to other schools in the district. Indeed, while the schools closed by Houston were generally lower performing than schools that did not close, they were not universally low performing in an absolute sense. Over four-fifths of the closed schools were rated as academically acceptable, with 10 rated as “recognized” or “exemplary”.

The finding that closures have a more pronounced impact on math achievement than reading achievement is perhaps not unexpected for several reasons. As a number of researchers have observed (Konstantopoulos, 2005, Nye, Konstantopoulos, & Hedges, 2004), while math learning takes place largely within the classroom, a larger proportion of reading learning occurs outside the classroom. As such, school and classroom effects on math achievement are likely to be larger than for reading achievement. Second, compared to reading instruction, there may be more variability in the curricular approaches and time devoted to math instruction (e.g., Konstantopoulos, 2005). Finally, a substantial literature has focused on the importance of establishing literacy skills by third grade; it is possible that the crystallization of reading ability at a relatively young age makes reading skills more stable and less amenable to the effects of closures (Center for Public Education, 2015).

Policy Implications

Although my findings suggest that school closures are not generally effective at improving the achievement of displaced students, it is important acknowledge that closures may be necessary in certain cases, particularly when enrollment declines are precipitous or when extreme low-achievement is chronic. Towards that end, I offer four key insights into more effective design of closure policies afforded by my findings. First, students fare best when they transfer to substantially higher-performing schools. While this point seems obvious, I think it is important to stress that the receiving school's performance must be *significantly* higher than that of the closed school to counteract the disruptive effects of closures. In the case of HISD, receiving schools need to perform over a quarter of a standard deviation above the district average.

This is a particularly important point for districts, such as HISD, that make the closing of racial/ethnic and socioeconomic achievement gaps a high priority. Chronically low-achieving and under-enrolled campuses are the primary targets of closure policy, and students of color, poor students, and low-achieving students, are disproportionately enrolled in such campuses. By failing to provide displaced students with high quality replacement campuses, districts can significantly undermine their own ability to narrow the persistent racial/ethnic and socioeconomic achievement gaps that plague public and private education in the U.S.

Second, to ensure that displaced students have access to high quality schools, I recommend that districts assign displaced students only to schools that are considerably higher-performing than the schools from which they are displaced. Moreover, for district with substantial school-choice options, such as HISD's magnet program, I would also recommend that displaced students be given preferential admissions to, or reserved slots in, the district's schools of choice. Importantly, when a school targeted for closure is not geographically located near substantially higher-performing schools, as is often the case, displaced students will likely have few nearby options to which they might transfer. In such cases, I believe other reform or turnaround techniques may prove substantially more effective than the closure of the campus.

Third, given the potential risk at which the forced mobility of a school closure places students, districts should carefully monitor the progress of and, when necessary, target interventions at displaced students. Results suggest that despite showing short-term gains in achievement, displaced students, particularly when transferring to relatively lower performing schools, tend to fall behind comparable, non-displaced students. As such, monitoring and intervention policies put in place to combat the negative effects of closure should continue beyond the year immediately following closure.

Finally, as discussed previously, school closures are often highly controversial, inciting strong resistance within the communities where closures are proposed (Fleisher, 2013; Hurdle, 2013). Moreover, across nearly all studies on school closures, including this one, research finds that closures disproportionately impact poor communities, and communities of color. This fact, regardless of the academic effects of closures, is highly concerning given the literature on the importance of public schools as anchor institutions within urban communities (Taylor, McGlynn, & Luster, 2013). That is, public schools, particularly in impoverished or underdeveloped neighborhoods, serve as important community organizations that often collaborate with a wide range of service providers (e.g., adult education, day care, youth services, etc.) to improve the conditions of life within their communities (Hudson & Holmes, 1994; Milner & Howard, 2004).

When viewed in this way, as vital community organizations (and organizers), it is not difficult to see why most school closure proposals are met with staunch resistance. Unfortunately, however, anecdotal evidence—cited mostly in newspaper articles—suggests that school boards and district policymakers often pay little attention to community dissent, even in the face of complaints to the Federal Office of Civil Rights (Hudle, 2013). Given the importance of public schools, particularly in underdeveloped communities, however, I recommend that school boards and school districts pay much closer attention to the requests and needs of community members. This recommendation is buttressed by the fact that most of research on closures indicates that, even when the effect of closures on achievement is positive, they are only modestly so. Moreover, available evidence suggests that closures are also not the financial windfalls that some proponents suggest them to be. Indeed, both Philadelphia and Washington, D.C. were unable to sell most of the vacated school properties from past rounds of closures, and those that did sell, sold for under market value (Brookings, 2009; Dowdall, 2011). As such, rather than closing schools within underdeveloped urban communities, perhaps school boards and district officials should work with community stakeholders to further enhance the schools role as an anchoring institution within the neighborhood.

Limitations & Future Directions

It is important to acknowledge a few key limitations of my analyses. First, it is important to consider that even if school closures have a negative impact on displaced students, it is not necessarily the case that the aggregate effect of closures is negative. Closures may certainly harm those students displaced and therefore directly affected by the closure. However, many other students have an indirect experience of a closure, by merely attending a different, more advantaged school rather than the original, less advantaged closed school they would have attended if not for the closure. For these students, there may be a positive effect of closure that is not captured in this and other studies of closures.

Second, while this study is narrowly focused on the impact of closures on student achievement, closures may have broader effects on students and their communities. Future research may seek to examine the impact of closures on student attainment, including dropout and graduation rates. Moreover, it is possible that the closure of schools, which often serve as hubs of community activity, may have broader impacts on the communities in which they are located. As such, future work should also focus on broader, neighborhood-level outcomes, such as changes in rates of crime, property values, and unemployment rates.

Third, although every effort was made to provide unbiased estimates of the impact of closures on student achievement via doubly-robust propensity score estimation methods and other robustness checks, it is possible that the displaced student sample differs systematically from the non-displaced group in terms of unobserved factors. Likewise, since closures are often announced, officially or unofficially, a year or more in advance, students remaining in the school the year immediately prior to a closure may differ systematically from students (and staff) electing to transfer prior to that year.

It is important to note, however, that Carlson and Lavertu (2015) examined the impact of closure announcements in their study of closures in Ohio and found little difference between models using two-years prior, and one-year prior to closure as the baseline year. Moreover, as discussed above, robustness estimates indicate that nearly three-fifths of the estimates' effects would need to be due to bias to invalidate my inferences. I do not believe that the timing of closure announcements, net of all the other variables that were included in my analyses, would account for such a large proportion of the estimated effects.

That said, the issue of the exact timing of closure announcements, and the related issues of why a school was closed, and how much resistance the closure proposal encountered are crucial aspects of the broader context that warrant further study. While it was beyond the scope of this analysis to collect historic and qualitative data regarding the broader context of the 46 school closures included in the sample, future work should focus on these issues to unpack the relationship between the academic outcomes of displaced students and the aforementioned contextual factors of closure policy.

Tables

Table 1

Basic Descriptives of HISD and Texas Public Schools, 2015-16

	HISD	Texas
Enrollment	214,891	--
% Black	24.0%	12.6%
% Hispanic	62.0%	52.2%
% White	8.5%	28.5%
% Asian	3.7%	4.0%
% Economically Disadvantaged	76.5%	59.0%
% English Language Learners	30.3%	18.5%
% At-Risk	64.2%	50.1%
Dropout Rate	3.9%	2.1%
% Passing meeting satisfactory standard (STAAR)	69.0%	75.0%

Table 2

Study Variables and Data Sources

	Description	Source
Phase I - Dependent Variable		
Experiencing a Closure	Categorical variable indicating if a student experienced a closure over the study period. Students displaced by closure were assigned a value of "1", while students never displaced by closure were assigned a value of "0".	PEIMS
Phase I - Independent Variables		
Student-Level Variables		
TAKS Scores - Mathematics	Student raw score on the Mathematics component of the TAKS.	TAKS Database
TAKS Scores - Reading	Student raw score on the Reading component of the TAKS.	TAKS Database
Age	Age of students at the beginning of a school year, measured in years.	PEIMS
At-Risk	Indicator variable identifying students that are at-risk for dropping out of school	PEIMS
Attendance	Continuous variable measuring the total number of days for which a student was present during the school year.	PEIMS
Economic Disadvantage	Indicator variable identifying students that are eligible for free/reduced price lunch or other public assistance programs.	PEIMS
Gifted/Talented (GT)	Indicator variable identifying students that are participating in state-approved GT programs.	PEIMS
Limited English Proficient (LEP)	Indicator variable identifying students that have limited English Proficiency according to the following two criteria: 1) a language other than English is used as the primary language in the home, and 2) the student's English language proficiency is determined to be limited by a Language Proficiency Assessment Committee (LPAC) or as indicated by a test of English proficiency.	PEIMS

Table 2

Study Variables and Data Sources

	Description	Source
Mobility	This set of variables indicates the cumulative number of school moves a student has made over their educational careers. This variable was constructed using the school enrollment information from the six 6-week enrollment periods frim the PEIMS data. Three types of mobility were calculated: 1) within-school-year mobility, which occurs when a student transfers to a new school during a school year, 2) between-school-year mobility, which occurs when a student transfers to a new school between school years, and is not in a terminal grade (e.g., 5^{th} grade or 8^{th} grade), and 3) structural mobility, which occurs when a student naturally transfers from elementary to middle school or from middle school to high school.	PEIMS
Race/Ethnicity	Students are recorded as belonging to one of five racial/ethnic groups: American Indian/Alaska Native, Asian/Pacific Islander, Black, Hispanic, or White.	PEIMS
Sex	The sex of students, recorded as female or male. Female students are assigned a value of "1" and male students are assigned a value of "0".	PEIMS
Special Education	This variable identifies students that are participating in special education services. These students have been identified as having at least one disability by an Individualized Education Program (IEP) committee.	PEIMS
School-level Covariates		
Achievement	The achievement of each campus in the sample is captured by two complimentary variables: the proportion of students annually that score at or above the satisfactory-level on TAKS mathematics and reading.	AEIS
Enrollment	This continuous variable captures the annual enrollment size (i.e., number of students) of each school in the sample.	AEIS
Phase II - Dependent Variables		
TAKS Scores - Mathematics	Student raw score on the Mathematics component of the TAKS.	TAKS Database
TAKS Scores - Reading	Student raw score on the Reading component of the TAKS.	TAKS Database
Phase II - Independent Variables		
Student-Level Variables		

Table 2

Study Variables and Data Sources

	Description	Source
School Closure	To quantify the effects of closure and reassignment on the academic trajectories of displaced students, the timing of school closures was tracked via two complementary variables. First, to quantify the immediate effects of closure on academic achievement a dichotomous variable was constructed that indicates when a student experienced a closure. Students are assigned a value of "0" in the years prior to experiencing a closure and a value of "1" in the years after they experience a closure. Second, to estimate the longer-term effects of closures on academic achievement, a continuous variable was constructed that tracks the number of years since a student experienced a closure. Prior to a closure and in the year immediately following a closure, the variable will take on a value of "0", and will then increase annually by a value of "1".	

Table 3

Characteristics of Closed Schools vs. Schools that Remained Open in HISD

	Closed		Open		Difference
	Mean	*SD*	Mean	*SD*	$X_{closed} - X_{open}$
Race/Ethnicity					
American Indian	0.02%	*0.09%*	0.11%	*0.24%*	-0.09%
Asian	0.92%	*2.72%*	2.79%	*5.99%*	-1.87%
Black	47.25%	*31.82%*	32.23%	*29.36%*	15.02%
Hispanic	49.58%	*31.10%*	58.63%	*29.95%*	-9.05%
White	2.22%	*4.99%*	6.23%	*12.14%*	-4.01%
Eco Disadvantage	90.66%	*13.01%*	82.06%	*20.57%*	8.60%
Gifted	3.33%	*4.91%*	11.85%	*14.60%*	-8.52%
LEP	28.08%	*28.98%*	30.79%	*23.53%*	-2.71%
Mobility	38.03%	*24.53%*	21.51%	*16.79%*	16.52%
Spec Ed	9.39%	*14.58%*	8.65%	*10.87%*	0.74%
Attendance	92.22%	*8.10%*	95.24%	*3.86%*	-3.02%
Enrollment	245.33	*359.85*	681.35	*472.64*	-436.02
Achievement					
Passing Math TAKS	58.43%	*22.44%*	79.02%	*16.99%*	-20.60%
Passing Reading TAKS	68.17%	*16.63%*	82.83%	*10.49%*	-14.67%
Teacher Characteristics					
First Year	7.57%	*9.12%*	6.41%	*5.50%*	1.16%
Years Experience	11.55	*3.55*	11.59	*3.27*	-0.04
Stud Teacher Ratio	14.09	*4.79*	15.77	*3.49*	-1.68
	N	%	N	%	$X_{closed} - X_{open}$
Level					
Elementary	34	73.91%	186	63.05%	10.86%
Middle	3	6.52%	51	17.29%	-10.77%
High	8	17.39%	50	16.95%	0.44%
Ungraded	1	2.17%	8	2.71%	-0.54%
Academic Rating[1]					
Unacceptable	7	18.92%	106	6.03%	12.89%
Acceptable	20	54.05%	866	49.29%	4.77%
Recognized	7	18.92%	542	30.85%	-11.93%
Exemplary	3	8.11%	243	13.83%	-5.72%
N Schools	46		295		
N Students	11,786		453,076		

[1] For closed schools, ratings reflect the rating in the year prior to closure. Ratings for schools that remained open reflect all ratings for the study period and therefore sum to more than the total number of schools.

Table 4

Balance Data for Pre- and Post-Matched Samples

	Pre-Match				Post-Match				
	Treatment Mean	Control Mean	Difference (Treatment - Control)	Standardized Mean Difference	Treatment Mean	Control Mean	Difference (Treatment - Control)	Standardized Mean Difference	% Bias Reduction
Propensity Score	0.016	0.004	0.012	0.674	0.016	0.016	0.000	0.013	98.11%
Covariates									
Student Level									
Age	13.316	12.452	0.863	0.250	13.328	13.254	0.074	0.022	91.37%
Female	0.499	0.489	0.010	0.011	0.499	0.508	-0.008	-0.017	-54.88%
Race/Ethnicity									
Asian	0.014	0.033	-0.019	-0.164	0.014	0.015	-0.001	-0.007	95.99%
Black	0.280	0.337	-0.057	-0.127	0.280	0.275	0.006	0.014	88.61%
Hispanic	0.684	0.538	0.146	0.315	0.683	0.684	-0.001	-0.001	99.57%
At-Risk	0.731	0.599	0.131	0.296	0.731	0.726	0.005	0.009	96.84%
Attendance	-10.503	0.043	-10.545	-0.233	-10.336	-10.363	0.028	0.001	99.73%
Eco Disadvantage	0.822	0.751	0.071	0.185	0.822	0.809	0.013	0.034	81.89%
LEP	0.212	0.191	0.021	0.051	0.211	0.203	0.008	0.020	60.25%
Gifted	0.080	0.101	-0.021	-0.078	0.080	0.085	-0.005	-0.019	75.99%
Special Ed	0.125	0.123	0.002	0.005	0.125	0.118	0.007	0.022	-340.16%
Mobility									
Between-Year	-0.098	0.000	-0.098	-0.122	-0.097	-0.023	-0.074	-0.092	24.37%
Within-Year	0.017	0.000	0.017	0.017	0.018	0.090	-0.072	-0.045	-168.16%
Structural	0.183	-0.001	0.184	0.214	0.184	0.307	-0.123	-0.143	33.14%
Achievement									
Math TAKS Raw	-1.040	0.004	-1.044	-0.107	-1.037	-0.024	-1.013	-0.104	3.00%
Reading TAKS Raw	-0.519	0.002	-0.521	-0.056	-0.501	0.049	-0.549	-0.059	-5.57%

Table 4

Balance Data for Pre- and Post-Matched Samples

	Pre-Match				Post-Match				
	Treatment Mean	Control Mean	Difference (Treatment - Control)	Standardized Mean Difference	Treatment Mean	Control Mean	Difference (Treatment - Control)	Standardized Mean Difference	% Bias Reduction
School Level									
Achievement									
TAKS Math	-0.146	0.001	-0.146	-0.734	-0.145	-0.128	-0.017	-0.086	88.26%
TAKS Reading	-0.066	0.000	-0.067	-0.472	-0.066	-0.046	-0.020	-0.142	69.87%
Enrollment	-83.357	0.338	-83.695	-0.078	-79.309	-100.852	21.543	0.020	74.25%
School Grade Span									
Elementary (K-5)	0.387	0.341	0.046	0.094	0.385	0.384	0.001	0.001	98.60%
Middle (6-8)	0.052	0.301	-0.249	-1.118	0.053	0.050	0.003	0.012	98.90%
High (9-12)	0.560	0.324	0.236	0.476	0.562	0.562	0.000	0.000	99.97%
Exact Matches									
Year	4.063	3.679	0.384	0.259	4.069	4.069	0.000	0.000	100.00%
Grade	7.636	7.019	0.617	0.197	7.649	7.649	0.000	0.000	100.00%
N Students/% Retained	6,855	331,665			6,829	6,829			99.62%

Table 5

Estimates of Closure Effect on Math TAKS from Multilevel OLS and PSM Regression Models

	OLS				Propensity Score Matched Estimates											
					No Covariates				Covariates				School Fixed Effects			
	Model 1				Model 2				Model 3				Model 4			
	b	SE	t		*b*	SE	t		*b*	SE	t		*b*	SE	t	
Intercept	15.144	0.115	131.92	*	20.851	0.227	91.7	*	11.635	1.487	7.83	*	12.605	22.961	0.55	
Year	1.946	0.009	214.98	*	1.18	0.135	8.77	*	1.996	0.049	40.69	*	1.55	0.049	31.53	*
Year^2	-0.152	0.001	-151.72	*	-0.017	0.006	-2.81	*	-0.132	0.006	-21.91	*	-0.041	0.006	-6.78	*
Close	0.45	0.095	4.75	*	0.177	0.153	1.15		0.684	0.138	4.94	*	0.702	0.152	4.6	*
Post-close	-0.248	0.09	-2.76	*	0.279	0.133	2.1	*	-0.174	0.128	-1.35		0.08	0.132	0.61	
Post-close^2	0.026	0.019	1.38		-0.182	0.031	-5.89	*	-0.069	0.03	-2.3	*	-0.137	0.031	-4.4	*
Grade	2.783	0.011	250.27	*	0.801	0.082	9.74	*	2.597	0.054	48.07	*	2.679	0.055	48.45	*
Student-Level Covariates																
Age	-1.597	0.011	-156.87	*	--	--	--		-1.532	0.048	-32.08	*	-1.528	0.049	-31.19	*
Female	-0.435	0.017	-26.05	*	--	--	--		-0.625	0.085	-7.38	*	-0.627	0.086	-7.26	*
Attendance	0.031	0.001	175.19	*	--	--	--		0.025	0.001	28.93	*	0.024	0.001	27.07	*
Gifted/Talented	4.187	0.024	174.66	*	--	--	--		4.188	0.127	32.96	*	4.235	0.129	32.79	*
Eco. Disadv.	-0.539	0.016	-33.71	*	--	--	--		-0.246	0.082	-2.99	*	-0.279	0.084	-3.34	*
At-Risk	-2.269	0.014	-163.69	*	--	--	--		-2.206	0.068	-32.58	*	-2.203	0.069	-31.69	*
LEP	-0.634	0.019	-32.02	*	--	--	--		-0.856	0.091	-9.46	*	-0.867	0.093	-9.37	*
SPECED	-4.958	0.022	-220.84	*	--	--	--		-4.846	0.111	-43.66	*	-4.872	0.115	-42.53	*
Asian	2.177	0.047	46.01	*	--	--	--		0.997	0.355	2.81	*	0.891	0.365	2.44	*
Black	-3.705	0.029	-129.59	*	--	--	--		-3.269	0.181	-18.02	*	-3.213	0.189	-16.94	*
Hispanic	-1.661	0.028	-59.67	*	--	--	--		-1.05	0.176	-6.27	*	-0.988	0.184	-5.37	*
Between-Year Mobility	0.063	0.009	6.56	*	--	--	--		-0.028	0.049	-0.56		-0.014	0.052	-0.26	
Within-Year Mobility	-0.559	0.009	-64.87	*	--	--	--		-0.624	0.043	-14.64	*	-0.618	0.045	-13.78	*
Structural Mobility	-0.243	0.015	-15.76	*	--	--	--		-0.183	0.078	-2.35	*	-0.009	0.083	-0.109	

Table 5

Estimates of Closure Effect on Math TAKS from Multilevel OLS and PSM Regression Models

	OLS				Propensity Score Matched Estimates									
					No Covariates			Covariates				School Fixed Effects		
	Model 1				Model 2			Model 3				Model 4		
	b	SE	t		*b*	SE	t	*b*	SE	t		*b*	SE	t
School-Level Covariates														
Middle School	1.882	0.076	24.79	*	--	--	--	2.932	0.276	10.62	*	--	--	--
High School	-0.288	0.068	-4.21	*	--	--	--	0.188	0.233	0.81		--	--	--
Attendance Rate	-1.548	0.434	-3.57	*	--	--	--	-4.249	1.379	-3.08	*	--	--	--
% Gifted/Talented	-2.393	0.171	-14.05	*	--	--	--	-2.455	0.572	-4.29	*	--	--	--
% Eco. Disadv.	1.462	0.093	15.79	*	--	--	--	1.513	0.355	4.26	*	--	--	--
% LEP	1.271	0.114	11.19	*	--	--	--	-0.038	0.317	-0.12		--	--	--
% SPECED	-0.311	0.201	-1.55		--	--	--	-1.816	0.718	-2.53	*	--	--	--
Mobility Rate	-0.192	0.115	-1.67		--	--	--	0.007	0.384	0.02		--	--	--
Student/Teacher Ratio	0.012	0.004	3.54	*	--	--	--	0.013	0.014	0.92		--	--	--
Teacher Yrs. Exper.	-0.011	0.003	-3.13	*	--	--	--	0.005	0.012	0.39		--	--	--
% White	-0.379	0.115	-3.29	*				-1.855	0.438	-4.23	*			
Enrollment	-0.0004	0.00003	-13.83	*	--	--	--	-0.001	0.0001	-5.91	*	--	--	--
% Met Math TAKS	10.097	0.074	136.42	*	--	--	--	10.559	0.303	34.82	*	--	--	--

Table 6

Estimates of Closure Effect on Reading TAKS from Multilevel OLS and PSM Regression Models

	OLS				Propensity Score Matched Estimates											
					No Covariates				Covariates				School Fixed Effects			
	Model 5				Model 6				Model 7				Model 8			
	b	SE	t		*b*	SE	t		*b*	SE	t		*b*	SE	t	
Intercept	8.31	0.093	89.71	*	12.375	0.224	55.23	*	3.805	0.461	8.25	*	8.43	8.481	0.99	
Year	1.546	0.008	192.78	*	1.567	0.044	35.72	*	1.91	0.043	44.35	*	1.769	0.045	39.65	*
Year^2	-0.124	0.001	-124.09	*	-0.105	0.006	-18.13	*	-0.149	0.006	-24.84	*	-0.117	0.006	-19.45	*
Close	0.091	0.088	1.03		-0.515	0.15	-3.43	*	-0.125	0.131	-0.95		-0.057	0.141	-0.41	
Post-close	-0.385	0.084	-4.57	*	-0.424	0.13	-3.27	*	-0.692	0.125	-5.52	*	-0.66	0.129	-5.11	*
Post-close^2	0.068	0.018	3.74	*	0.079	0.029	2.7	*	0.149	0.028	5.27	*	0.134	0.029	4.59	*
Grade	3.958	0.01	394.69	*	2.512	0.027	93.2	*	3.914	0.048	82.32	*	4.008	0.049	82.2	*
Student-Level Covariates																
Age	-1.332	0.009	-149.82		--	--	--		-1.201	0.042	-28.92		-1.196	0.042	-28.18	
Female	0.772	0.014	56.25		--	--	--		0.616	0.068	9.01		0.609	0.069	8.76	
Attendance	0.019	0.0002	118.38		--	--	--		0.015	0.001	19.02		0.014	0.001	17.62	
Gifted/Talented	2.984	0.021	140.01		--	--	--		2.828	0.113	24.94		2.845	0.115	24.78	
Eco. Disadv.	-0.867	0.014	-59.86		--	--	--		-0.687	0.075	-9.15		-0.692	0.076	-9.06	
At-Risk	-1.252	0.013	-98.81		--	--	--		-1.292	0.062	-20.83		-1.305	0.063	-20.56	
LEP	-2.599	0.018	-147.26		--	--	--		-3.268	0.081	-40.47		-3.349	0.082	-40.71	
SPECED	-5.151	0.019	-265.13		--	--	--		-5.295	0.095	-55.62		-5.302	0.098	-54.37	
Asian	0.165	0.041	4.12		--	--	--		0.158	0.309	0.51		0.018	0.317	0.058	
Black	-1.939	0.025	-79.08		--	--	--		-1.669	0.158	-10.59		-1.657	0.165	-10.07	
Hispanic	-1.249	0.024	-52.21		--	--	--		-0.861	0.153	-5.62		-0.793	0.159	-4.96	
Between-Year Mobility	0.108	0.008	12.94		--	--	--		0.099	0.043	2.29		0.108	0.045	2.39	
Within-Year Mobility	-0.295	0.007	-39.51		--	--	--		-0.337	0.037	-9.07		-0.348	0.039	-8.95	
Structural Mobility	-1.135	0.013	-85.53		--	--	--		-0.931	0.067	-13.95		-0.742	0.069	-10.65	
					--	--	--									

Table 6

Estimates of Closure Effect on Reading TAKS from Multilevel OLS and PSM Regression Models

	OLS			Propensity Score Matched Estimates								
				No Covariates			Covariates			School Fixed Effects		
	Model 5			Model 6			Model 7			Model 8		
	b	SE	t	*b*	SE	t	*b*	SE	t	*b*	SE	t
School-Level Covariates												
Middle School	3.452	0.069	49.39	--	--	--	5.005	0.262	19.07	--	--	--
High School	1.316	0.064	20.69	--	--	--	2.083	0.226	9.23	--	--	--
Attendance Rate	-2.064	0.402	-5.14	--	--	--	-0.119	1.324	-0.09	--	--	--
% Gifted/Talented	-0.798	0.157	-5.08	--	--	--	0.154	0.551	0.28	--	--	--
% Eco. Disadv.	1.426	0.085	16.76	--	--	--	0.885	0.337	2.63	--	--	--
% LEP	1.107	0.104	10.59	--	--	--	0.763	0.307	2.48	--	--	--
% SPECED	-0.358	0.185	-1.93	--	--	--	-1.234	0.691	-1.79	--	--	--
Mobility Rate	-0.924	0.107	-8.64	--	--	--	-0.285	0.369	-0.77	--	--	--
Student/Teacher Ratio	0.003	0.003	0.97	--	--	--	-0.019	0.013	-1.44	--	--	--
Teacher Yrs. Exper.	-0.028	0.003	-8.86	--	--	--	-0.032	0.012	-2.75	--	--	--
% White	0.396	0.105	3.76	--	--	--	-1.082	0.409	-2.65	--	--	--
Enrollment	-0.0003	0.00003	-9.06	--	--	--	-0.0002	0.0001	-1.92	--	--	--
% Met Reading TAKS	7.194	0.087	82.67	--	--	--	7.749	0.363	21.33	--	--	--

Table 7
PSM Regression Models with Student Race/Ethnicity Interaction Terms

	Math Model 9				Reading Model 10			
	b	SE	t		*b*	SE	t	
Intercept	11.633	1.492	7.80	*	3.784	0.467	8.10	*
Year	1.998	0.049	40.67	*	1.915	0.043	44.47	*
Year^2	-0.132	0.006	-21.97	*	-0.150	0.006	-25.01	*
Close	-0.166	0.688	-0.24		-0.562	0.404	-1.39	
Post-close	0.824	1.064	0.77		0.220	0.653	0.34	
Post-close^2	-0.104	0.175	-0.60		0.023	0.144	0.16	
Grade	2.593	0.054	47.95	*	3.913	0.048	82.33	*
Black	-3.099	0.258	-12.02	*	-1.636	0.202	-8.09	*
Hispanic	-0.820	0.265	-3.09	*	-0.763	0.199	-3.84	*
Close X Black	1.176	0.673	1.75		0.213	0.519	0.41	
Postclose X Black	-1.339	1.214	-1.10		-0.640	0.727	-0.88	
Postclose^2 X Black	0.120	0.186	0.65		0.085	0.155	0.55	
Close X Hispanic	0.705	0.693	1.02		0.603	0.521	1.16	
Postclose X Hispanic	-0.920	1.102	-0.83		-1.161	0.683	-1.70	
Postclose^2 X Hispanic	0.008	0.181	0.05		0.166	0.148	1.12	

Note. For the sake of parsimony, I omit most of the coefficients for the student- and school-level covariates in this table. The full array of covariates, however, were used to estimate these models.

Table 8

PSM Regression Models with Student Economic Disadvantage Interaction Terms

	Math Model 11				Reading Model 12			
	b	SE	t		*b*	SE	t	
Intercept	11.650	1.500	7.77	*	3.798	0.462	8.21	*
Year	1.998	0.049	40.71	*	1.911	0.043	44.36	*
Year^2	-0.132	0.006	-21.97	*	-0.150	0.006	-24.90	*
Close	0.024	0.292	0.08		-0.254	0.367	-0.69	
Post-close	0.641	0.339	1.89		-0.329	0.328	-1.00	
Post-close^2	-0.210	0.074	-2.85	*	0.079	0.070	1.13	
Grade	2.396	0.291	8.23	*	3.914	0.048	82.32	*
Economic Disadvantage	-0.617	0.097	-6.36	*	-0.642	0.087	-7.37	*
Close X Eco. Disadv.	0.079	0.129	0.61		0.238	0.302	0.79	
Postclose X Eco. Disadv.	-1.297	0.385	-3.37	*	-0.436	0.359	-1.21	
Postclose^2 X Eco. Disadv	0.169	0.082	2.06	*	0.084	0.076	1.10	

Note. For the sake of parsimony, I omit most of the coefficients for the student- and school-level covariates in this table. The full array of covariates, however, were used to estimate these models.

Table 9

PSM Regression Models with School Quality Interaction Terms

	Math Model 13				Reading Model 14			
	b	SE	t		*b*	SE	t	
Intercept	11.577	1.453	7.97	*	3.801	0.462	8.23	*
Year	1.982	0.049	40.3	*	1.911	0.043	44.37	*
Year^2	-0.129	0.006	-21.37	*	-0.15	0.006	-24.9	*
Close	0.561	0.17	3.29	*	-0.149	0.137	-1.09	
Post-close	0.135	0.235	0.57		-0.754	0.133	-5.66	*
Post-close^2	-0.114	0.038	-2.99	*	0.176	0.032	5.52	*
Grade	2.593	0.054	48.01	*	3.914	0.048	82.36	*
School TAKS	10.357	0.362	28.62	*	7.934	0.46	17.25	*
Close X School TAKS	-1.181	0.694	-1.7		-1.436	1.776	-0.81	
Post-close X School TAKS	3.138	1.056	2.97	*	2.901	3.518	0.82	
Post-close^2 X School TAKS	-0.371	0.233	-1.59		-0.805	0.489	-1.65	

Note. For the sake of parsimony, I omit most of the coefficients for the student- and school-level covariates in this table. The full array of covariates, however, were used to estimate these models.

Table 10

Proportion of displaced students transferring to schools of given quality level, by student characteristics

	Receiving School Quality			
	>=0.8 SD Below Average[1]	>=0.27 SD Above Average[2]	>=1 SD Above Average	<=1 SD Below Average
All	0.905	0.374	0.207	0.075
Asian	0.948 [b]	0.671 [b,h,w]	0.448 [b,h]	0.000
Black	0.856	0.513 [h]	0.235 [h]	0.092 [a,w]
Hispanic	0.924 [b]	0.304	0.186	0.070 [a,w]
White	0.926 [b]	0.492 [h]	0.344 [b,h]	0.049
Eco Disadvantage	0.903	0.383 [*]	0.204	0.075
Non-Eco Disadvantage	0.918	0.323	0.228	0.072
At Risk	0.895	0.336	0.173	0.086 [*]
Non-At Risk	0.928 [*]	0.467 [*]	0.291 [*]	0.047
Female	0.899	0.364	0.203	0.077
Male	0.911	0.385	0.211	0.072
Gifted	0.965 [*]	0.453 [*]	0.319 [*]	0.014
Non-Gifted	0.899	0.367	0.196	0.081 [*]
LEP	0.924 [*]	0.454 [*]	0.256 [*]	0.070
Non-LEP	0.899	0.350	0.192	0.076
Spec Ed	0.888	0.360	0.176	0.086
Non-Spec Ed	0.907	0.377	0.212 [*]	0.073
Met Math TAKS Standard	0.943 [*]	0.471 [*]	0.286 [*]	0.033
Did Not Meet Math TAKS Standard	0.873	0.218	0.098	0.116 [*]

[a,b,h,w] Racial/ethnic group has statistically higher probability of attending school of a given quality than groups denoted by superscript: a = Asian, b = Black, h = Hispanic, w = White.

[*]Group has statistically higher probability of attending school of a given quality.

[1] For student experiencing closure after 4th grade, between 2004 and 2005, school quality necessary to produce higher than expected achievement in 5th grade.

[2] For student experiencing closure after 4th grade, between 2004 and 2005, school quality necessary to produce higher than expected achievement in 10th grade.

References

Alexander K., Entwisle, D., & Dauber, S. (1996). Children in motion: School transfers and elementary school performance. *The Journal of Educational Research, 90*(1), 3-12.

Austin, P. (2011). An introduction of propensity score methods for reducing the effects of confounding in observational studies. *Multivariate Behavioral Research, 46*, 399-424.

Austin, P., Grootendorst, P., & Anderson, G. (2007). A comparison of the ability of different propensity score models to balance measured variables between treated and untreated subjects: A Monte Carlo study. *Statistics in Medicine, 26*, 734-753.

Brookhart, M., Schneeweiss, S., Rothman, K., Glynn, R., Avorn, J., & Stürmer, T. (2006). Variable selection for propensity score models. *American Journal of Epidemiology, 163*, 1149-1156.

Brookings Institution (2009). Analysis of the impact of DCPS school closings for SY2008-2009. Washington, D.C.

Brown, E. (2015, April 28). School closings are politically difficult. But are they good for student achievement? *The Washington Post*.

Brummet, Q. (2012). The effect of school closings on student achievement. *Journal of Public Economics, 119*, 108-124.

Burdick-Will, J., Keels, M., & Schuble, T. (2013). Closing and opening schools: The association between neighborhood characteristics and the location of new educational opportunities in a large urban district. *Journal of Urban Affairs, 35*, 59–80.

Carlson, D. & Lavertu, S. (2016). Charter school closure and student achievement: Evidence from Ohio. *Journal of Urban Economics, 95*, 31-48.

Carlson, D. & Lavertu, S. (2015). *School closures and student achievement: An analysis of Ohio's urban districts and charter schools*. Thomas B. Fordham Institute: Columbus, OH.

Carr, S. (2013, March 8), Does closing underperforming schools help or hurt students? *The Atlantic*.

Center for Public Education. (2015). *Learning to read, reading to learn: Why third-grade is a pivotal year for mastering literacy*. Center for Public Education: Alexandria, VA.

Consortium on Chicago School Research (2009). *When schools close: Effects on displaced students in Chicago public schools.* Consortium on Chicago School Research. Chicago, IL.

Corley, C. (2013, March 22). Chicago teachers, parents riled by plan to close 54 public schools. *NPR Education*.

de la Torre, M. & Gwynne, J. (2009). *When schools close: Effects on displaced students in Chicago public schools*. Consortium on Chicago School Research: Chicago, IL.

de Souza Briggs, X. (2007). More pluribus, less unum? The changing geography of race and opportunity. In X. de Souza Briggs (Ed.), The geography of opportunity: Race and housing choice in metropolitan America (pp. 17–44). Washington, DC: The Brookings Institution.

Dowdall, E. (2011, October). *Closing Public Schools in Philadelphia: Lessons from Six Urban Districts*. The Pew Charitable Trust: Philadelphia, PA.

Duffin, E. (2009, March). Displaced kids doing poorly. *Catalyst*.

Engberg, J., Gill, B., Zamarro, G., & Zimmer, R. (2012). Closing schools in a shrinking district: Do student outcomes depend on which schools are closed? *Journal of Urban Economics, 71*(2), 189-203.

Fleisher, L. (2013, January 28). School closures challenged. *The Wall Street Journal*.

Frank, K. (2000). Impact of a confounding variable on a regression equation. *Sociological Methods and Research 29*, 147-194.

Frank, K. A., Maroulis, S. J., Duong, M. Q., & Kelcey, B. M. (2013). What would it take to change an inference? Using Rubin's causal model to interpret the robustness of causal inferences. *Educational Evaluation and Policy Analysis, 35*(4), 437-460.

Hanushek, E., Kain, J., & Rivkin, S. (2004). Disruption versus Tiebout improvement: The costs and benefits of switching schools. *Journal of Public Economics, 88,* 1721-1746.

Hirano, K. & Imbens, G. (2001). Estimation of causal effects using propensity score weighting: An application to data on right heart catheterization." *Health Services and Outcomes Research Methodology,* 2, 259-278.

Hirano, K., Imbens, G., & Ridder, G. (2003). Efficient estimation of average treatment effects using the estimated propensity score. *Econometrica 71*, 1161-1189.

Houston Independent School District (HISD). (2014). *Student classification, promotion standards*. Student Guidelines 2014-15. Houston Independent School District: Houston, TX. Retrieved from http://www.houstonisd.org/site/handlers/filedownload.ashx?moduleinstanceid=157796&dataid=111909&FileName=Section7%201415%20B.pdf

Houston Independent School District (HISD). (n.d.). School histories database. Retrieved from http://www.houstonisd.org/Page/32479

Honaker, J., King, G., & Blackwell, M. (2015). AMELIA II: A program for missing data. Retrieved from https://cran.r-project.org/web/packages/Amelia/vignettes/amelia.pdf

Hudson, M.J. & Holmes, B.J. (1994). Missing teachers: impaired communities: The unanticipated consequences of *Brown v. Board of Education* on the African American teaching force at the precollegiate level. *The Journal of Negro Education, 63*(3), 388–393.

Hurdle, J. (2013, January 28). Education department to hear school closing complaints. *The New York Times.*

Jack, J. & Sludden, J. (2013). School closings in Philadelphia. *Penn GSE Perspectives on Urban Education, 10*(1).

Joffe, M., Ten Have, T. , Feldman, H. , & Kimmel, S. (2004). Model selection, confounder control, and marginal structural models: Review and new applications. *The American Statistician, 58,* 272-279.

Kirshner, B., Gaertner, M., & Pozzononi, K. (2010). Tracing transitions: The effect of high school closure on displaced students. *Educational Evaluation and Policy Analysis, 32(3)*, 407-429.

Konstantopoulos, S. (2005). Trends of school effects on student achievement: Evidence from NLS: 72, HSB: 82, and NELS: 92. IZA Discussion Paper 1749. IZA: Bonn, Germany.

Lipman, P., & Person, A. S. (2007). Students as collateral damage. *A preliminary study of the Renaissance 2010 School Closings in the Midsouth*.

Mehana, M. & Reynolds, A. (2004). School mobility and achievement: a meta-analysis. *Children and Youth Services Review*, *26*, 93-119.

Mid-Atlantic Equity Center. (2009). *Does closing schools cause educational harm? A review of the research*. Bethesda, MD.

Milner, H.R. & Howard, T.C. (2004). Black teachers, Black students, Black communities, and Brown: Perspectives and Insights. *The Journal of Negro Education*, *73*(3), 285–297.

Morgan, S., & Harding, D. (2006). Matching estimators of causal effects: Prospects and pitfalls in theory and practice. *Sociological Methods and Research 35*, 3–60.

Morgan, S., & Todd, J. (2008). A diagnostic routine for the detection of consequential heterogeneity of causal effects. *Sociological Methodology*, *38*, 231-281.

Murnane, R.J, & Willett, J.B. (2011). Methods matter: Improving Causal Inference in Educational and Social Science research. New York, NY: Oxford University Press.

Nye, B., Konstantopoulos, S., & Hedges, L. V. (2004). How large are teacher effects? *Educational Evaluation and Policy Analysis, 26*(3), 237–257.

PEW Charitable Trust (2011). *Closing public schools in Philadelphia: Lessons from six urban districts*. Philadelphia, PA.

PEW Charitable Trust (2013). *Shuttering public schools: The struggle to bring old buildings to new life*. Philadelphia, PA.

Pribesh, S. & Downey, D. (1999). Why are residential and school moves associated with poor school performance. *Demography, 36*(4), 521-534.

Rosenbaum, P., & Rubin, D. (1983). Assessing sensitivity to an unobserved binary covariate in an observational study with binary outcome. *Journal of the Royal Statistical Society, Series B, 45*, 212-218.

Rubin, D. B. (1987). Multiple imputation for nonresponse in Surveys. New York, NY: Wiley.

Rubin, D. (2001). Using propensity scores to help design observational studies: Application to the tobacco litigation. *Health Services & Outcomes Research Methodology, 2*, 169-188.

Rubin, D. B. (2004). The design of a general and flexible system for handling nonresponse in sample surveys. *The American Statistician, 58*(4), 298–302.

Rumberger, R. (2003). The causes and consequences of student mobility. *Journal of Negro Education, 72*(1), 6-21.

Rumberger, R., & Larson, K. (1998). Student mobility and the increased risk of high school dropout. *American Journal of Education, 107*(1), 1-35.

Sacerdote, B. (2012). When the saints come marching in: Effects of hurricanes Katrina and Rita on student evacuees. *American Economic Journal: Applied Economics. 4* (1), 109–135.

Sixel, L.M. (2015, June 20). Population growth is fueling Houston's economy. *Houston Chronicle*.

Schwartz, A. E. & Stiefel, L., & Cordes, S. A. (2015). *Moving matters: The causal effect of school mobility on student performance*. Working Paper 01-15. Institute for Education and Social Policy: New York, NY.

Shadish W. R., Clark, M. H., & Steiner, P. M. (2008). Can nonrandomized experiments yield accurate answers? A randomized experiment comparing random to nonrandom assignment. *Journal of the American Statistical Association, 103, 1334–1343*.

Singer, J. & Willett, J. (2003). *Applied longitudinal data analysis: Modeling change and event occurrence*. London: Oxford University Press.

Steiner, L. (2009). *Tough decisions: Closing persistently low-performing schools*. Center on Innovation and Improvement: Lincoln, IL.

Stuart, E. A. (2010). Matching methods for causal inference: A review and a look forward. *Statistical science: a review journal of the Institute of Mathematical Statistics*, *25*(1), 1.

Sunderman, G. L., & Payne, P. (2009). *Does closing schools cause educational harm? A review of the research*. Mid-Atlantic Equity Center: Bethesda, MD.

Swanson, C., & Schneider, B. (1999). Students on the move: Residential and educational mobility in America's schools. *Sociology of Education, 72*(1), 54-67.

Temple, J., & Reynolds, A. (1999). School mobility and achievement: Longitudinal findings from an urban cohort. *Journal of School Psychology, 37*(4), 355-377.

Taylor, E. S. (2014). Spending more of the school day in math class: Evidence from a regression discontinuity in middle school. *Journal of Public Economics, 117*, 162-181.

Taylor, H. L., McGlynn, L., & Luter, G. (2013). Back to the future: Public schools as neighborhood anchor institutions—the choice neighborhood initiative in Buffalo, New York. In K. Patterson & R. Silverman (Eds.), Schools and urban revitalization: Rethinking institutions and community development (pp. 109-135). New York, NY: Routledge

Van Buuren, S., Boshuizen, H. C., & Knook, D. L. (1999). Multiple imputation of missing blood pressure covariates in survival analysis. *Statistics in Medicine, 18*, 681-694.

Winters, M. (2010). Measuring the effect of charter schools on public school student achievement in an urban environment: Evidence from New York City. *Economics of Education Review, 31*, 293-301.

Xu, Z., Hannaway, J., & D'Souza, S. (2009). *Student transience in North Carolina: The effect of school mobility on student outcomes using longitudinal data.* Working Paper 22. National Center for the Analysis of Longitudinal Data in Education Research: Washington, DC.